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PLANS FOR RECONSTRUCTION IN GREAT BRITAIN

THE dangers which follow on the Government's delay in announcing its decisions on the Uthwatt and Scott Reports and in laying down the broad lines of its planning policy become almost daily more apparent. The scope and imaginative courage of the plan for the County of London prepared for the London County Council (see p. 227) is one of the strongest of a number of recent challenges to action. The plan itself is still under discussion by the Council, but obviously it cannot be given proper consideration without reference to other planning authorities. At almost every point regional considerations arise; often they predominate, as, for example, in projects affecting the distribution of industry and of the industrial population.

It is not merely that the size and wealth of the London County Council ensure that its proposals must exert an important influence on the plans of the many smaller authorities. The distribution of open spaces, the layout of roads and railways, the shopping centres, markets and theatres may be largely domestic matters. The grand features of the plan—the dispersal of large numbers of population and the controlled location of industry—are matters of regional and national concern, and their determination involves decisions of national policy. Publication of the plan at the present moment has provided an outstanding example of the way in which the absence of authoritative decisions by the Government creates formidable difficulties in the preparation of reconstruction plans by local planning authorities.

No planning for the location of industry, either in the form of the dispersal of industry and population from congested areas or in the form of the direct provision of industrial employment where it is needed, is possible before the Government has made three fundamental decisions: on the Barlow Commission's proposals for the decentralization of industry; on the Scott Committee's qualifying proposals for the control of construction in rural areas; and on those of the Uthwatt Committee regarding the methods of land control by which the direction of industry and construction are to be effected. The development rights scheme dealing with undeveloped land and the proposals for strengthening public powers of acquisition in urban areas may not indeed be sufficient in the great urban problems such as that of London, as the Uthwatt Report itself indicates. There is, however, no indication of official initiative or of any national economic policy in these vital matters. Without such a national policy adequate schemes cannot be prepared, as the London plan itself demonstrates conclusively. Local planning is at a standstill until the national policy is made known. No lip service to reconstruction will clear the Government of something more than a suspicion of deliberate obstruction if this position persists.

A further difficulty exemplified in the London plan is also emphasized in Guy Hunter's "The British

Way in Local Rule",* and in a recent "Occasional Paper" of the National Institute of Economic and Social Research† by Prof. J. R. Hicks and U. K. Hicks. In the latter paper, which gives a further example of the important contribution which the Institute is continuing to offer to social science and reconstruction, Prof. J. R. Hicks and U. K. Hicks present part of the results of an inquiry into the burden of local taxation which they are conducting on behalf of the Institute. Although this investigation of county boroughs in Britain was limited to an attempt to supply answers to two questions of fact, its conclusions as to policy and as to the functions of local government, together with its observations on the Beveridge Report, give it an importance highly relevant in the present connexion regarding the relations between central and local government in planning.

To the two questions of fact the report returns a straight answer. The main reason for disparity in rates is disparity of wealth. A poor town cannot get enough revenue to reach an average standard demanded by public opinion, backed by direct and indirect pressure of the central government, without imposing a crushing poundage. A rich town can reach the same standard without any strain. As regards the recrudescence of the rate problem, the general growth in the nation's economic prosperity during the nineteen-thirties led very naturally to an increase in local expenditure, which in the fields of health and education left a larger share of the increase to be borne by the rates. The decease of the old Poor Law also placed unforeseen responsibilities on the local authorities, but the revolution in the standards of public assistance was by far the most important factor in the renewed pressure on the rates. To preserve the national resilience against future economic shocks by preventing such possibilities before they occur, the authors suggest that the endemic crisis of public assistance could be ended in a way altogether consistent with present aspirations for social security and also profoundly consistent with the natural evolution of English institutions by a National Board of Guardians to deal with domiciliary relief.

Once again, therefore, we are brought face to face with the need for a declaration of general policy in regard to local government reform, and this in a field closely touching that covered by the Beveridge Report. Moreover, Messrs. Hicks make some pertinent observations on local government generally which accentuate the need for decision. Local government needs to be equitable financially, but it should also be an effective organ of democracy, and this it clearly is not as we have it at present. Again, it is no longer necessary as formerly for such matters as relief to be local responsibilities. Messrs. Hicks point out that we are freer than our ancestors to choose what functions should be allotted to local government, and that we should make our choice, not merely on grounds of efficiency in administration but also with a view to the possibility of using local government to give practice in the working of democracy to far wider circles than can be brought into any intimate contact with the national parliamentary organization.

This goes to the roots of the matter, and no passage in this valuable study deserves to be more widely pondered than that in which the authors indicate criteria on which such a choice could be made. In particular, they refer to the opportunity for achievements of a distinctly local character offered by the field of 'civic amenities' in its broadest sense, including not only highways, housing, parks and public lighting, but also town planning and public buildings. Nor is this all. Analysis of the subject from a point of view totally different from that of Sir William Beveridge has led to the same conclusion as to the urgency of transferring public assistance to central control. This paper, however, directs attention to the dangers which would arise if the Beveridge proposals were enacted and no alteration made in the financial relations between the central government and the local authorities. If the Social Security Plan is not to be, in one important respect, a sham, its cost must be distributed fairly, not only between classes but also between localities. Furthermore, apart from the suggestion, to avoid this danger, of a national rate, emphasis is once more laid on the necessity of a proper regional co-ordination of hospital services. The present extremely haphazard development of such services involves grave danger of disproportionate burdens being placed on the poorer authorities.

The same necessity for finding at the centre the ground plans of policy and the solution of legal and financial obstacles is stressed by Guy Hunter in his pamphlet on the "British Way in Local Rule". For many of the proposed reforms of education, transport and public utility services, as in planning, the areas of operation differ widely from the present local government areas. This administrative difficulty must be faced if we are to draw effectively on the local councils for their experience, leadership and goodwill in the execution of policy. The London plan itself both affords evidence of the value of local tradition and experience and challenges the Government to declare its mind and policy. Paralysis, if not chaos, must speedily result if we are left much longer in doubt as to whether a general reform of local government areas and powers is in prospect, or whether it is proposed to tackle planning first, grafting a new pattern of planning authorities on to the existing structure of local government.

The challenge to Government and Parliament to decide and act on the fundamental assumptions on which the London plan rests is as unmistakable as the challenge which the plan offers to the imagination and resource of London's citizens. Fully as emphatically, however, the recent debate in the House of Commons on the Forestry Commission showed the dangers which have attended the failure to plan with foresight, determination and consistency in the period since the War of 1914–18. The recent report of the

* The British Way in Local Rule. By Guy Hunter. (Oxford Pamphlets on Home Affairs, No. H (8).) (London: Oxford University Press, 1943.) 6d. net.

† Standards of Local Expenditure: A Problem of the Inequality of Incomes. By J. R. Hicks and U. K. Hicks. (National Institute of Economic and Social Research, Occasional Papers, No. 3.) Pp. 61. (London: Cambridge University Press, 1943.) 4s. 6d. net.

Forestry Commission puts the problem of afforestation, agriculture and rural reconstruction in its right perspective but, as the report itself indicates and as Mr. Clement Davies reminded the House of Commons, in spite of a parallel report twenty-five years ago, immediately the danger was passed, we reverted to our old chaotic methods.

There can be no question that in this particular matter the Government is right in proceeding with caution, and the assurance of financial continuity in the vigorous execution of whatever policy is finally adopted is all that could reasonably be expected at the moment. Afforestation is essentially a problem demanding a long-term policy of assured continuity. It has to be related to agricultural policy—its relation to hill-grazing requirements was repeatedly stressed in the debate, and Sir William Jowitt intimated that the Government is awaiting reports on the hill sheep industry—and a long-term policy for agriculture has yet to be formulated. Again, afforestation, like the larger question of agriculture, is linked with the broad question of export and import policy in our post-war trade, and no continuous long-term policy can be determined without full consideration of these questions also.

The report itself received some criticism as well as praise in the debate and, whatever its intrinsic merits, the scheme proposed is confused in aim and gives no clear guidance as to the real objectives of afforestation policy. There was, however, general support for the view that there should be a more effective central administration of forestry, whether or not it was placed under the Minister of Agriculture, and less reliance on, or tenderness towards, private owners than the report itself suggests. Sir William Jowitt assured the House that forestry would be considered along with post-war commitments as a whole, and that pronouncement on policy would not long be delayed.

With that assurance there would be no reason for dissatisfaction, but for the Government's own record in the field of reconstruction policy. It has to be admitted that some of the matters raised in the Forestry Commission's report, as was noted in the debate, touch on the proposals of the Uthwatt Report in regard to the ownership of land. Others affect the policy of rural reconstruction raised by the Scott Report. Once again we come back to the central point at issue, that over the whole intricate field of post-war reconstruction certain broad and basic decisions of policy are required of the Government before policy can be formulated, nationally or locally, in particular fields, and plans prepared for the execution of such policy regionally, locally, or in specialized fields. Appreciation of the complexity of the situation and the general interlocking of the many post-war problems is widespread and tempers what might otherwise be a dangerous impatience with the Government's delay. If, however, that delay continues indefinitely, impatience may pass into a sense of frustration, with disastrous consequences not only to the war effort but also to our chances of establishing the harmonious co-operation of all the many elements in our institutions—local government, central administration, Parliamentary oversight and technical and professional advice, and above all the keen interest of the average citizen in local rule—on which depend our hopes of building a worthy post-war world.

RECLAIMING THE GREAT WREN

County of London Plan

Prepared for the London County Council by J. H. Forshaw and Prof. Patrick Abercrombie. Pp. xii+188+59 plates. (London: Macmillan and Co., Ltd., 1943.) 12*s.* 6*d.* net.

SELDOM in the history of Great Britain can such a gigantic scheme of land reclamation have been put forward as a practicable proposal; seldom can such a case have been made out for powers to alter the physical environment of a great population. The book of the County of London Plan is first and foremost a quite remarkable essay in public relations, in the right kind of publicity for the special aspect of democratic action which we call 'town and country planning'. Published at a price which must be suited to the purchaser rather than the promoter, beautifully set-out and illustrated, and finely printed, with large coloured maps, descriptive photographs, and a host of diagrams and line drawings, the book is the best ambassador to the public that the plan could have. Neither Moscow nor New York were surveyed so ably and in so popular a format, and only Amsterdam can claim to have produced anything comparable to it. It is not possible to read it without becoming deeply interested in the future of London, as in its past; and in a world of war-time restrictions, this achievement of authors, publishers, printers, illustrators and engravers is, in itself, a matter for congratulation.

With such a complex subject, expert presentation is something to be thankful for; the gist of the matter is nevertheless far from easy to analyse. Clearly the main contribution which the plan makes is its imaginative grouping of the residential neighbourhoods and the commercial and administrative centres, of which London is composed. Its most important proposal is therefore one of density, that is to say, the size of the population and its disposition within the county boundaries, the numbers that are being—or should be encouraged to be—moved out. The elements that must contribute to the realization of such a comparatively stabilized pattern, namely, the roads and railways, the location of workplaces and the provision and maintenance of open space, are all secondary to the social reorganization which is the basis of the plan.

Before examining the road or rail proposals, therefore (which are in fact described as tentative, and for the consideration of a "specially-appointed investigating body"), one must consider the essential principles involved in the proposed re-grouping of the population. These principles are in harmony with enlightened thought in the realms of housing, social science and public administration; nevertheless the consequences of their application in any particular case are liable to be intensely controversial. It may mean flats instead of houses, footways instead of traffic approaches, restriction to a local rather than a central shopping area, or a reduction in the time of journeys to work. Briefly, the plan contemplates "the conservation or creation of communities which

would be divided into smaller neighbourhood units of between 6,000 and 10,000 persons, related to the elementary school and the area it serves". Parallel with this the plan proposes three standards of density, reasonable for the central, sub-central, and suburban areas to which they would be applied, namely, 200, 136 and 100 persons to the average acre. These standards are then compared with existing densities, the area in each case being composed of the houses, their gardens and yards, the service and access roads, and half the main roads bounding the site. The excess represents the number to be decentralized from each area; and, allowing for variations in the national population trends, the total for the County of London is estimated to be about half a million.

This establishment of a standard of density, and its use as a measuring rod to determine the degree of congestion, is a difficult business. The authors of the London plan have very courageously proposed these compromise figures, as suitable to the particular case of London, capable of being met without the necessity for perpetrating retrograde housing conditions, and in proportion to their expectations of the corresponding decentralization of industry. Many people will view the compromise as an unhappy one; they will take the view that ideal housing conditions —which means in effect a house and garden for at least 90 per cent of all populations, including that of London—should determine the standard of density, and that industry should be compulsorily decentralized to enable this standard to be carried into effect. Such criticism is logical, but scarcely realistic. As it is, the figures proposed are an enormous advance on present conditions. The number to be housed at a density of 200 per acre (which comes down to less than 100 to the acre when open spaces on the scale of 4 acres per 1,000 of the population are included in the calculation) is only the central 300,000; and most of those are now housed at almost double the density. Mr. Forshaw and Prof. Abercrombie consider that reasonable conditions of access, lighting, open space and so forth, would only be obtained by housing people in flats at this density; but Mr. Trystan Edwards has recently written to *The Times* to state that it could be done with two- or three-floored terrace houses. The intermediate density of 136 would normally require two flats to every house (though again Mr. Edwards states that it could be met by bungalows!); and the lower density of 100 would normally have more houses than flats.

In spite of the formidable opposition already raised against the standards proposed in the London plan, it seems likely that they will find general support; unless really drastic and far-reaching legislation is introduced to bring the ideal nearer to realization.

On the 'community' principle there can scarcely be grave disagreement. Traffic safety, social organization, economy, and neighbourliness, all call for it as an element of town improvement. The amazing biological plate facing p. 21 in the book of the London plan shows the social and functional analysis of the communities of the metropolis. Whether the proposed solution is quite functional enough, or whether an intermediate grouping is desirable between the many reconstructed units and the central areas, will only be proved by time. The main theme is a London reintegrated, and not a London rubbed out and redrawn.

Probably the most unconvincing feature of the plan is that of industrial location. It is unconvincing because even the crowded area of the London County Council is a *local* area, and the industry of London is *national* in character. Certain preliminary facts are sorted out, some of them rather surprising; as, for example, that the average number of workers per factory in the County was only 20 in 1938, and that of 37,000 factories and workshops two-fifths were in the clothing industry. The drift from central to less central areas in search of cheaper land, less congestion and better opportunities for expansion, is clearly faced; and in general the plan proposes that industry, which is anyway on the move, should be encouraged to move to the right places, if not to industrial areas prepared and serviced for its reception.

The analysis does not go very far, and much remains to be done in calculating the degree of mobility of the industrial complex, its sex and skill ratios, and the extent to which a system of inducement, coupled or not with licensing powers, would bring about the desired pattern shown tentatively on the coloured plate No. 11. More still depends on the policy which the Government adopts when eventually it faces the issues of the Barlow Report, and the extent to which it is prepared to introduce legislation to regulate post-war industrial settlement. But since the standards of density which are at the root of the L.C.C. proposals are based on an estimated rate of industrial decentralization, this is evidently a question of major importance, and one which the plan does not—and probably cannot—answer.

The other proposals are all susceptible to change in detail. The roads are mainly Bressey roads, with additions in the shape of through cross-roads meeting in the heart of London and reaching it partly by tunnel. It is always doubtful whether road improvements may not create as much traffic as they facilitate; and here again everything depends on the extent to which the neighbourhood pattern, imaginative zoning, and the estimates of industrial movement prove adequate to the occasion, and reduce the total amount of unnecessary traffic. The distribution system by an "outer goods ring railway", feeding both in and out by road transport, is a useful suggestion. So is the loop line which joins London Bridge, Cannon Street, Blackfriars, Charing Cross and Waterloo, which it is hoped may eventually be put underground and thus eliminate the cross-river railway bridges. This proposal is similar to that put forward by Westminster in evidence to the Royal Commission on Cross River Traffic in 1926; and since forgotten. But all the railway proposals are tentative, and much will depend on the proposals of the railway companies themselves, if and when these are made public.

For the rest, one can only recommend a study of the many fascinating details which the plan discusses, such as the "precincts" of Westminster and Bloomsbury, the relation of shops to road planning (p. 57), the open space proposals (which would remove once for all the worst features of overcrowding in the most congested areas), the riparian treatment of the Thames, the discussion on "period planning" (p. 145), and the very important Appendix I, giving the figures for decentralization.

However specialists may criticize the plan for failure to clarify particular issues, the fact remains that this proposal puts the whole of the L.C.C. reconstruction scheme into scale and into the proper perspective. It is an enormous and an ambitious challenge to public and to Government alike, to face up to the cost and labour that will be required, and to grasp the inspiring opportunity that is presented to them between the covers of this book.

ARCHÆOLOGICAL FINDS FROM GREECE AND CRETE

The Annual of the British School at Athens
No. 40: Session 1939–40. Pp. iv+87+32 plates. (London: Macmillan and Co., Ltd., 1943.) 42*s.* net.

IN spite of the difficulties of the times, the British School at Athens has produced its fortieth Annual, and the only marks of trouble are the reduced number of pages and a paper cover instead of the usual boards. The number of plates and text figures is generous, and the contents very well indeed up to the mark.

The article of perhaps the most general interest is Mr. Heurtley's on the School excavations in Ithaca, with his summary of conclusions. Guided by the survival of the name, the general reader has always been content to regard the modern Ithaca, or Thiaki, as the island immortalized in the Odyssey, and if the details of the topography could be shown not to fit very exactly in all points, he reassured himself by thinking that Homer was writing an epic poem and not a topographical treatise and, in any event, he had his headquarters somewhere in the eastern part of the Ægean and a very long way from the island of Odysseus: he aimed, that is, at no more than a general local colour which would pass muster with his hearers. But professional topographers, beginning with Gell, had always felt these difficulties and doubts, and the question came very much to the fore with the activities of Dr. Doerpfeld, who loudly maintained that the real Ithaca of Homer was the island of Leucas, and claimed that his excavations there had proved his view. It seems not to have struck him that, as Odysseus was only one of a number of island chieftains, it is hard to see how any discovery of remains of the required period could clinch the question. Then the British School began to excavate in Ithaca, and found that it had been inhabited all through the required period, and thus, quite apart from tradition, was no worse off as a candidate for Homeric honours than any other of the neighbouring islands. Finally, the School found a broken mask of the first century A.D. inscribed with the name of Odysseus, to say nothing of potsherds bearing the word *nymphs*. Mr. Heurtley sums up the whole question in his final remark, a model of common sense: "To the doubt as to how Odysseus' name came to be associated with Ithaca, the answer is perhaps the simple one, that it was really his home, and it was there that the events took place which supplied the material for Homer's description of the Return."

Of the other articles—there are in all seven—two deal with East Crete. One by Mr. Hutchinson, Miss Eccles and Miss Benton, is devoted to some objects still left unpublished from the excavations at Praisos and Palaikastro, which began so long ago as 1902. The other is on Dicte and the Temple of Dictæan Zeus; it is a memorandum written before 1910 by the late Prof. R. C. Bosanquet, which seems to have been intended as a historical introduction to the definite report of these excavations at Praisos and Palaikastro, of which this volume contains the aftermath. It is a learned topographical and archæological study of the whole of East Crete, the Sitein Peninsula, written by one who knew the country well, and it was due to his memory that it should be published. In it are embedded notes of a visit which he paid in 1903 to the island of Kouphonisi off the southern coast of the peninsula, with an account of the remains, Minoan and later, which he found there. Mr. Hutchinson has enriched this study with a note: Where was Dicte?

Other articles are an epigraphical study by J. M. R. Cormack, "Royal Letters in Beroea", a paper by Miss Benton on the dating of early Greek armour, and a long paper by Anton E. Raubitschek, "Early Attic Votive Monuments". This has an interesting note on the sculptor Euenor, the master of Pheidias, and a statue of Athena which might be a work of Euenor, or even (but here the author expresses himself with great caution) of Pheidias himself in his young days.

The last article is by Prof. J. D. Beazley: "An Archaic Greek Statuette from South Arabia". A British officer was riding in the Aden Protectorate and was offered a bronze statuette, five inches high, of a warrior, wearing corslet, helmet and greaves. To buy it was not possible, but a photograph was secured, and an enlargement is here reproduced. The work has every appearance of genuineness, and Prof. Beazley says it would be "good Greek work from the middle of the third quarter of the sixth century B.C.; Peloponnesian and very possibly Laconian". Nothing else of this sort seems to have been found in Arabia, and it well illustrates the never to be forgotten maxim that single finds of almost any sort of object may turn up almost anywhere: certainly few are more astonishing than this one. Apparently the tribesman still has it, unless he has found a passing customer with more ready money in his pocket.

In conclusion: though the volume is slim, the quality is high, and the British School at Athens is to be heartily congratulated on its appearance.

R. M. DAWKINS.

THE FREE WILL CONTROVERSY

The Free Will Controversy
By Dr. M. Davidson. Pp. ix+118. (London: Watts and Co., Ltd., 1942.) 7*s.* 6*d.* net.

A FEW years ago Dr. Davidson published a work entitled "Free Will or Determinism", which was devoted almost entirely to an exposition and discussion of the bearing of modern science on one of the oldest and most perplexing of philosophical problems. Some readers at least felt that the effectiveness of this work suffered somewhat from the lack of a historical setting, and this lack has now been supplied in the volume under review. In a small compass the thoughts of men on the free-will problem have been traced from the early Babylonian astrologers, through Greek, Roman, Hebrew and Christian philosophy, the doctrines of Descartes, Spinoza, Leibniz, Hume, Kant, Mill and Haeckel, to their present preoccupation with the implications of physics and biology. There is a "Conclusion", rightly so called in the sense that it ends the work, but containing no attempt to sum up the evidence or to pronounce a verdict. The treatment throughout is strictly objective and non-partisan, the writing is clear and interesting, and the volume forms a necessary and very useful supplement to the earlier book.

It would be easy but unjust to stress the fact that important omissions have been made. The author is well aware of this, and has, in addition to the obvious defence of its intrinsic inevitability, the further defence that paper restrictions imposed a maximum size on the book. Considering the limits allowed him, it is difficult to see how Dr. Davidson could have made a better choice of material, except that some reference might have been made to the effect on general thought—an effect not completely represented in the standard philosophies of the period—of the mechanistic character of physical science during the eighteenth and nineteenth centuries, for it is only in relation to this that the claims made that modern physics has emancipated us from the chains of "necessity" have any significance. The argument was, briefly, that the movements of matter had been proved to be controlled by universal laws, that human actions were movements of matter, and that therefore human actions were controlled by laws which operated independently of the will of the human being. It was a very weak argument at the best, for the one recognizable distinguishing characteristic of living matter was that it did *not* move according to the laws found valid for non-living matter. Nevertheless, the prospect seemed to cause serious perplexity, and the modern discovery that the laws of motion of non-living matter cannot be effectively used for conceptual entities like electrons unaccountably gave great comfort by making it seem possible that, after all, our bodies after we are dead might not be quite inevitably under the control of physical law. Why this is more consoling than the obvious fact that they are not so controlled while we live would be a suitable theme for Dr. Davidson's next book if psychology is included among his many interests.

H. DINGLE.

THE COMING-OF-AGE OF INDUSTRIAL PSYCHOLOGY

An Introduction to Industrial Psychology
By May Smith, D.Sc. Pp. 264. (London, Toronto, Melbourne and Sydney: Cassell and Co., Ltd., 1943.) 7*s.* 6*d.* net.

PRESENT-DAY psychology can be understood only in its historical setting, which Dr. May Smith outlines in her first chapter, for modern advance in science had strengthened an attitude—even now persistent in some degree—unfavourable to psychological ways of thought. Stahl, "the last of the animists", possessed a knowledge of psychology that had to be rediscovered in our day, and with his defeat the subject was banished from medicine, the branch of science that needs it most. Yet it would not keep out. A century after Stahl there came Mesmer with his theories of magnetic fluids to account for psychological phenomena; another century passed and Charcot, with his *grande hysterie* and metallotherapy, was discredited in his turn. Freud at last provided an understanding of these and other clinical problems, but we were still in the dark age of psychology, and not until the War of 1914–18, with its thousands of breakdowns, was medicine compelled to acknowledge its existence. On the academic side psychology had been a branch of philosophy, but workers like McDougall, with his postulation of instincts as a driving force, were approaching a common ground with Freud; the two lines of descent came together and psychology became an integral part of medicine, at the same time finding wide application outside that field.

The author comments, after referring to an early writer on what we now call vocational guidance: "One cannot but regret that this part of his work did not receive the attention it deserved, but the 17th century had something else to consider, and, caught by the exciting drama of the rapid advances in chemistry and mechanics, paid little heed to the suggestions of a writer whose interest was in men's minds".

What was true in the seventeenth century was true in the nineteenth. Industry had grown up under economic laws that, though some of their results were softened by legislation, were accepted as not only necessary but also sufficient to ensure the efficiency of the worker. His happiness and contentment were his own private affairs. There was no room for psychology here, but again the War of 1914–18 led to a fundamental change, and under its stimulus was set up the Health of Munition Workers' Committee, which gave origin to the Industrial Health Research Board and marked the beginning of great developments in the study of human problems in industry. In these developments Dr. May Smith, who had been a pupil and fellow-worker of McDougall, has taken an important part, and few can be better qualified to write a survey of this many-sided subject.

Fatigue, the first subject of systematic study, can be measured by tests and estimated by output figures, but, in bringing earlier work into relation with experience after Dunkirk, Dr. Smith strikes a note that prevails throughout the book, and tells us what the workers feel and think. "We shall have to pinch time off if we don't get it" is a sentiment the psychologist would teach us to accept as a factor in absenteeism. Moreover, the ability to listen easily to the expression of such sentiments is essential in the study of problems that appear to be purely physiological—such as lighting, temperature, noise, rest pauses, and the like. An account is given, with references, to work done on these topics, and on accident proneness, training, and vocational guidance and selection. The chapter on time- and motion-study gives full credit to its originator, F. W. Taylor; but it is noted that his work was subjected to severe criticism, which arose from his frank acceptance of American economic theory and practice as they existed in 1911, and found its focal point in the phrase 'the number of men required was reduced'. Dr. May Smith's thesis is that industrial psychology gives first consideration to the happiness and well-being of the worker, productive efficiency being their natural sequel.

Dr. May Smith's own work led her to the study of the nervous temperament and its influence upon efficiency and sickness, which she describes with a lively human touch. The medical reader will find here a fuller treatment of the subject than his own text-books provide, a treatment aided by unobtrusive psychological knowledge combined with wide experience. The book is an authoritative and up-to-date account of the whole subject, and not only satisfies an urgent need in our present trials but also offers valuable guidance in those new adjustments that will be inevitable when the War shall have ended.

MILLAIS CULPIN.

EDMOND HALLEY AND GEOMAGNETISM*

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MY subject is really Halley's geomagnetic work considered in its historical setting, but I wish first to say a little about Halley as poet—a seldom remembered aspect of his remarkable versatility.

Halley's Latin Verses

Halley is known to have written three poems, all in Latin†. The first and major poem, consisting of forty-eight hexameters, was written when he was thirty or thirty-one. It is in praise of Isaac Newton, and was prefixed to the "Principia", which Halley published at his own expense and care. As Prof. H. C. Plummer showed in his Halley Lecture[1] last year, the supreme merit of Newton's work was not immediately and universally recognized. But Halley was in no doubt, and we may imagine his joy and satisfaction in being privileged to evoke Newton's immortal work and to bring it before the world. The hexameters are written in this spirit. In the second edition of the "Principia", edited by Roger Cotes, Bentley amended some of Halley's lines, without leave; in the third edition, edited by Pemberton in 1726, Halley restored most of the original readings[2].

As Latin is now less commonly understood than in Halley's time, I venture to give an English version of this poem made by my wife. This is naturally less concise than the original; the forty-eight Latin lines become sixty-eight in the English.

The poem mentions many of Newton's main discoveries, and describes how Newton has enlarged man's powers and raised him still further above the animals. Finally, it acclaims Newton himself as having done more for mankind than any of its moral and social leaders or the discoverers of the useful arts, placing him of all men next to the Deity.

To the Honour of
this mathematical-physical work
of the most excellent
ISAAC NEWTON
a signal glory of
our Century
and of our human
Race.

Behold, to thee the Rule that guides the Pole,
To thee the balance of the Massive Whole,
To thee the Skies, Jove's holy seat sublime,
Count out their numbered path through Space and Time.
By what Set Laws the All-Father has confined
His own Creative Act thou hast divined,
Hast shewn wherein the Great Creator laid
The Firm Foundation for the world he made.
The Inmost Secrets of the Conquered Sky
Lie open to thy wise discerning eye;
And that same Wisdom all undaunted probes
The Force that spins around the furthest globes.
The Sun commands all things, descending prone
To rush towards him, seated on his throne;
The Starry Chariots suffers not to trace
Straight Paths through the Enormous Void of Space;
Himself the Centre, drawing each, he sways
The Ordered Circuit of their Starry Ways.
Known now the Path the Dreadful Comets weave;
The Bearded Stars no more our minds can grieve.
Wherefore the Silver Moon with varying gait
Speeds on, from thee we learn, though learning late;
Know how she hid, until thy wiser days
The secret of the Curb which She obeys.
We learn from thee wherefore her nodes regress
And why her apses ever forward press.
When Wandering Cynthia doth the sea compel
How Great the Force thy doctrine now can tell,
Measure her Strength, when tired waves leave the shore
And show the sailor sands he guessed before,
Or when, in turn, returning on their way
They seek the utmost marge, beneath her sway.
Those problems which the ancient scholars vexed
Those noisy contests which the Schools perplexed
Plainly we solve—thy lore dispels the cloud
And by dull error now no head is bowed.
For us thy wit has trod Celestial Ways
And shown the steps of heaven to our gaze.
Mortals, arise! shake off your earthly cares
This Man our Heavenly Origin declares
For fellow minds of such a Master Mind
Are far removed from all the Brutish Kind.
Less did they raise to better things our life,
Our human life, who taught that bloody strife,
That theft, adultery and fraud's wicked guile
Are cursed, and wrote their Lore with graver's stile.
Less he, who to fierce tribes that wandered rude
Taught city life and their wild fears subdued,
Bade them live safe, held in a wall's strong arm.
Less he, who taught the hungry folk to farm.
Less he, who pressed with new devised craft
From the sweet grape her care-dispelling draught.
Less he who, plucking the Nile-watered reed,
Made from it paper, wrote thereon his screed
To painted picture linking spoken sound.
All these for human woes great solace found,
But we of Superhuman Joys are free.
We know the Law that guides the poles, we see
The hidden secret of dark earth unsealed
And Matter's Changeless Ordering revealed.
Such knowledge, hidden from an earlier age,
Is now, through thee, to us an open page.
O Muses, sing with me this Great Man's praise
Ye who are fed with nectar, tune your lays.
Newton, to Truth's locked Shrine has found the Key,
Newton whose heart is pure, in whom we see
The Godhead's Favoured Friend to whom is given
The Nearest Place a man may win to Heaven.

Halley may have written another poem[3], now lost, in the year 1687 when the "Principia" was published. His two other known poems appeared thirteen years later, on his famous world magnetic chart; this, too, must have been for him a time of great joy in achievement. One poem was in praise of Queen Anne, and is now of little interest; the other, as follows, of much greater artistic merit, lauded the unknown inventor of the compass[4].

Him who first taught with magnetism to imbue
The iron: and the ocean's watery waves
Made clear to ships erst doubting: him who linked
Shores, tell his time far sundered, and by wind
Brought mutual products to remotest lands.

* Halley Lecture delivered at Oxford on May 28.

† Halley also used Latin in his first paper in the *Philosophical Transactions of the Royal Society*, in his Catalogue of the Southern Stars, and occasionally in later papers. Hevelius, in a letter of July 5, 1670, to the Royal Society, published in the *Phil. Trans.* for that year (pp. 2059–2061), opens with the sentence: "Would to God, that those Excellent Books that are publish'd in English, were, for the benefit of the whole Learned World, made Latin."

A thankless day, a heedless age have hid.
No mighty name survives him, being dead.
Hope not to wrest thy fame from Stygean shades
Nor seek to win thy ashes honours due.
And yet—to know within thy secret heart,
A shell surpassing common mortals, to have blessed
The life of far-off grandsons, is not this
Itself the Elysian fields, the shining crown?

The Magnet and the Compass

The science of geomagnetism has a long history. In our time its earlier phases have been studied especially by Crichton Mitchell[5], and in the preceding generation by Hellmann[6] and Sylvanus Thompson[7].

The attraction of iron by the magnet or loadstone was known to antiquity, and is mentioned by Plato and many later writers[7]. The directive property of the magnet was discovered much later; Crichton Mitchell[5a], after reviewing the available evidence, concludes that it was certainly known to the Chinese in the eleventh century, and in Europe in the twelfth. A Chinaman, Shon-Kua (A.D. 1030–93), wrote that "fortune-tellers rub the point of a needle with the stone of a magnet in order to make it point south". Before 1200 Alexander Neckam, a monk of St. Albans, described the mariner's compass, in which a magnetic needle swings on a pivot, as being in common use.

The compass points south, said the Chinaman, north, said the Europeans; both were right. Though the choice is arbitrary, it affected men's ideas as to the cause. In Europe the Pole Star was often supposed to attract the north end of the needle.

Nowadays a boy's first lesson on magnetism describes the dipolarity of magnets, the north and south poles (named by means of the directive property), the repulsion of like poles and the attraction of unlike. Petrus Peregrinus in 1269 gave the first-known systematic record of these facts, in a letter[8] from Italy to a home friend in Picardy. Manuscript copies spread the knowledge; the letter was not printed until 1558. Though it includes a description of an ingenious magnetic perpetual motion machine, the letter is generally clear and modern in spirit; Petrus wrote from actual experiment, mostly using loadstones cut to the spherical form.

Magnetic Declination

The recognition of the magnetic declination* as a general fact of Nature came only gradually. The declination of the compass from the true north cannot be measured without first determining the meridian at the place of observation, and this requires skill. At first the declination was ascribed to faults in the magnetic needle or in the measurement. Even in 1701 two needles used by Halley on his magnetic voyages gave readings of the declination at London that differed by 20′; in the fifteenth and sixteenth centuries the errors in magnetic azimuths might be a few degrees. Nowadays at magnetic observatories the declination is read[9] to 0·1′, though this requires great skill and care.

Our earliest dated records of the magnetic declination are not written ones. In the fifteenth century, portable sun-dials[10] were used by travellers and others to tell the time; they included a magnetic compass by which to set the noon line. Some dial maker who had determined his true meridian must have noticed that the magnetic setting was erroneous, so he put a mark on his dials to which the needle should point. The angle between this mark and the north point of the dial showed the declination, as reckoned by the maker. Dated dials made at Nuremberg at about 1450 show that the declination there was then about 7° E.

A little later, before 1500, road maps made by Etzlaub of Nuremberg[11] bear instructions for their use in association with the traveller's compass, and show clearly that the declination was known. A compass card printed on one such map has an arrow pointing 11¼° E. (one-eighth of a quadrant, the smallest unit then commonly used in stating wind directions).

Crichton Mitchell[12] has carefully examined the statements often made that Columbus, on his West Indian voyage in 1492, discovered the declination or the fact that it is not everywhere the same (ref. 6a). He concludes adversely on both points, stating that Columbus misinterpreted the differences observed between his compass readings; he also rejects similar claims made on behalf of the Cabots.

Measuring the Declination

The first printed instructions for measuring the declination were given in the earliest printed treatise on navigation, by a Portuguese writer Falero[13], in 1535. They were mainly astronomical, to determine the true meridian. Magellan seems to have taken a manuscript copy of this book with him in 1519 on his voyage round the world. The first notable series of forty-three observations of the declination were made by the great Spanish navigator de Castro, in a voyage to the East Indies and the Red Sea, 1538–1541. The custom spread to later navigators, and knowledge of the declination in various seas slowly grew.

But this knowledge was very imperfectly diffused, and many landsmen and seamen in other countries remained ignorant of the methods used and the observations made by the navigators of Spain and Portugal. Most writers on magnetism and dials up to 1600 fail to refer even to the existence of the declination[14].

The earliest known written record of the declination gives it for Rome, 6° E., in 1510. It occurs in a letter[15] sent by Georg Hartmann in 1544 to Duke Albrecht of Prussia; the measurement was made while Hartmann, a mathematician, instrument maker, and later vicar of a Nuremberg church, was studying in Rome. In Nuremberg, when he wrote, he found the declination to be 9° or 10° E.

This letter contains also the first mention of the magnetic dip. It describes how a needle perfectly balanced before it was magnetized dipped about 9° after being stroked by the loadstone. Hartmann contented himself with this private mention of his observation, which he regarded as one more wonder and mystery of the magnet; the dip must really have been much more than 9°. The letter remained unknown in the Königsberg archives until 1831.

In 1581 Robert Norman, a London seaman and instrument maker, published "The Newe Attractive", a small book said by Hellmann to be the first printed work purely on geomagnetism. It announced the discovery[16], probably in 1576, of the magnetic dip, which Norman gives as 71° 50′.

The book also describes new experiments of great interest. Norman showed that, when care is taken, a floating compass is not drawn bodily northward, but only turns on its centre; hence, said he, earlier writers erred in discussing where lies the *point*

* Or magnetic *variation*, as it is called by seamen and many old writers on geomagnetism.

attractive of the compass; the compass is not attracted to any point, but there is a point which it respects and turns to; so he discarded the old term *point attractive* for his new term *point respective*.

In making this important distinction he did not discriminate between the whole compass and its northern end, to which he, like earlier non-Chinese writers, gave a preference. As the northern end dipped, he located the point respective in the earth.

Important ideas were struggling to birth in the mind of this practical seaman and mechanic, more discerning than Hartmann. He was right in concluding that the earth, not the heavens, directed the magnetic needle; but his argument was faulty in that it depended on an unconscious preference for the north end of the needle. The idea of a couple acting on the needle still lay far in the future, beyond even Halley a century later.

William Gilbert, Queen Elizabeth's physician, laid a firmer foundation for Norman's conclusion. In the great treatise "De Magnete" (1600), Gilbert announced that the earth itself is a great magnet. This book, written in Latin, describes a great variety of his own experiments, and clearly distinguishes between magnetic and electric attractions. Gilbert chiefly used spherical loadstones, like Petrus Peregrinus three and a half centuries before; he carefully explored how tiny magnetic needles, free to turn on their centres, set themselves when placed at different points near such a stone or terrella. He found that at the 'equator' of the stone, midway between its poles, the needle would lie along the surface, whereas nearer the poles one or other end would dip; by means of the needles, he marked out on his terrella, with chalk, the magnetic meridians or lines of horizontal magnetic force, converging to the two poles.

He pointed to the analogy between the poleward direction of these little needles on the sphere, and the northward direction of compass needles on the earth. He fortified the analogy by Norman's observation of the magnetic dip*, roughly equal to the dip of his own little needle on the sphere, when placed in a similar 'latitude'. As the magnetism of the terrella obviously controlled the direction of his needles, he inferred, as Norman had done, but now with more reason, that the earth controls the direction of the compass or dip needle. Going beyond Norman, he realized that the earth itself is a great magnet.

Thus he showed that the directive property of the magnet, which hitherto had been regarded as a phenomenon distinct from its attractive or repulsive action, was really only a manifestation of the same power, in relation to the earth magnet, the bigness of which had until then hindered this recognition.

Gilbert commended Norman's rejection of the old idea of a *point attractive*, but himself rejected Norman's *point respective*, because the little needles, placed at different points on the terrella, met in no common point. The whole earth, he said, exerts the directive power, not merely the centre or any other single point.

Distribution of the Declination

Falero, in the treatise already mentioned, stated without any warrant from observation that the magnetic declination was distributed regularly over the earth. At that time the declination was zero at one of the Azores. Falero said that, going eastward from this meridian, the declination would be east, and would regularly increase over the first 90° of longitude; thence it would regularly decrease to zero in the next 90° of longitude; in the 180° of west longitude the declination would be westerly and would vary in the same way.

Gilbert knew that its distribution was less regular than this. He quoted (Bk. IV, Ch. 1) the declination at London and elsewhere, and concluded that the earth's directive power was defective over the oceans (water being non-magnetic) and stronger where the land was elevated. He regarded these inequalities as disturbing the tendency of the needle towards the magnetic poles, which he supposed to coincide with the geographical poles. Halley afterwards disproved Gilbert's theory, showing that at sea the needle may point away from an adjacent continent.

Gilbert and the Magnetic Dip[17]

Gilbert also erred like Falero in thinking that the earth's magnetism is more regular than is the fact; but Gilbert's error concerned not the declination but the dip. He supposed that the dip depends only on the latitude, and would enable seamen to determine their latitude when clouds covered the sky. He gave an empirical theory for the magnitude of the dip, and Briggs of Gresham College, at Gilbert's suggestion, calculated a table of dip and latitude on this theory. Kircher, a Jesuit writer on magnetism, in 1643 gave a table[18] of computed and observed dips. Further discovery showed that the method was impracticable.

It is remarkable, in view of the importance of the dip in Gilbert's demonstration that the earth is a great magnet, that he quoted not one actual observation of it, neither Norman's nor any made by himself, though he invented a simplified portable dip circle[19].

Gilbert made the further unfounded assertion that at any one place the declination remains constant, unless changed by some great cataclysm like the fabled submergence of Atlantis. This was disproved in 1634 by Gellibrand[20], Gresham professor, who found that the declination at London was then only 4·1° E., quite irreconcilable with the value 11·3° E. measured in 1580 by William Borough[21].

Thus the secular magnetic variation came to light, which imposes on us the duty, both for science and practice, continually to repeat the magnetic survey of the globe. The earth's magnetism has changed greatly in the three centuries since then, and its future, uncertain and mysterious, remains to be observed by later ages.

Halley and his Magnetic Needle

The story of magnetic discovery has now brought us to the time of Halley[22,23], who was born in 1656[24]. While still at St. Paul's School, he decided to devote his life to astronomy, which he did to such effect that his name will ever live in the annals of that science. But geomagnetism was one of his favourite secondary interests. In 1672, while only sixteen and at school, he measured the magnetic declination in London; it was 2° 30′ W., showing a further westward change of 6½° since Gellibrand's observation. The laying down of the meridian for this measurement fitted in well with Halley's astronomical interests.

At Oxford, Halley planned an expedition across the equator to observe the southern stars, and in 1676 he was enabled to go to St. Helena, through the good offices of the Royal Society, King Charles II.

* Gilbert called the declination the *variation*, and the dip the *declination*.

and the East India Company. There he spent a year (1676–77) most fruitful for astronomy. He took his magnetic needle with him, besides his astronomical equipment, and observed the declination[15,16] at St. Helena and Ascension.

Not long after his return to England and the publication of his results, he paid an astronomical visit to Hevelius* at Danzig, where again he appears to have used his compass[15]. A little later he set out from England to make the grand tour with a friend; while in Paris and Rome he observed the declination[16]. In London, at Gresham College, then the home of the Royal Society, he often repeated his measurement[17], for example in 1683, 1692, 1701, 1702, and 1716. In 1696, when he went to Chester as deputy controller of the Chester Mint, he again took his magnetic needle with him[16]. These observations clearly show his long-continued interest in the earth's magnetism.

Halley's Theory of the Variation (Declination)

In 1682 Halley married and settled down in London. The next year, when he was twenty-seven years of age, the Royal Society published his famous theory of the distribution of the declination[18]. His aim was "to reconcile the observations by some general rule", not, as Des Cartes[19] did, by "causes altogether uncertain (as are the casual lying of iron mines and loadstones in the earth)", which to Halley seemed to "put a stop to all further contemplation, and give discouragement to those that would otherwise undertake this enquiry".

"Tis true," he said, "that not long since one Mr. *Bond*[20], an old teacher of navigation, put forth a small treatise wherein he pretends to calculate the variation." Bond's theory merely supposed the magnetic axis of the earth to be oblique to the geographical axis, so that the declination could be readily calculated anywhere. Halley saw that this theory requires the declination to have the same sign all along each geographical meridian, and showed that this did not agree with observation[21].

Halley then gave a table of dated observations of the declination, made by "persons of good skill and integrity", at forty-eight places in many parts of the globe. He showed that these did not confirm Gilbert's theory that at sea the compass would always turn to an adjacent mainland. As to Des Cartes, Halley admitted that some local irregularities of the declination, like those near Elba, might be explained by local deposits of iron ore; but he said that no reasonable amount of it could explain why the needle declines the same way all over great areas such as the whole Indian Sea. "Besides, against both Des Cartes and Gilbert, the change of the variation, which has been within this hundred years last past more than 15 gr. at London, is an entire demonstration: tho Des Cartes does not stick to say, that the transportation of iron from place to place, and the growth of new iron within the earth, where there was none before, may be the cause thereof."

Halley himself, "after a great many close thoughts", and wishing to "introduce nothing strange in philosophy", could "come to no other conclusion than that, *The whole globe of the earth is one great magnet, having four magnetical poles, or points of attraction, near each pole of the equator two, and that, in those parts of the world which lye near adjacent to any one of these magnetical poles, the needle is governed thereby, the nearest pole being always predominant over the more remote*".

Halley gave the positions of the four poles, "as near as conjecture can reach", "for want of sufficient data to proceed geometrically"; and he indicated the four regions, each governed mainly by one of these poles. He ended with the following admirably moderate remarks[22] on the declination:

"But to calculate exactly what it is, in any place assigned, is what I dare not yet pretend to; . . ., for first there are a great many observations requisite. . . . And besides it remains undetermined in what proportion the attractive power decreases, as you remove from the poles of a magnet; without which it were a vain attempt to go about to calculate. There is yet a further difficultie, which is the change of the variation, one of the discoveries of this last century: which shows, that it will require some hundreds of years to establish a compleat doctrine of the magnetical system . . . it should seem, that all the magnetical poles had a motion westward: but if it be so, tis evident that it is not a rotation about the axis of the earth . . . But whether these magnetical poles move altogether with one motion, or with several, . . . are secrets as yet utterly unknown to mankind; and are reserved for the industry of future ages."

Halley's Experiments on the Law of Magnetic Attraction

In 1687 Halley made experiments[23] to determine "in what proportion the attractive power decreases, as you remove from the pole of a magnet"; this was one of the needs that hindered the completion of his theory. But his work was inconclusive; the discovery of the inverse square law of force between point magnetic poles did not come until a century later. In Halley's time several necessary theoretical conceptions were wanting—the point pole, the point dipole, the magnetic moment, the couple. Newton might have developed these had he given his close attention to magnetism; but Halley, who did so much in connexion with Newton's "Principia"*, seems never to have inspired Newton with his own strong interest in magnetism.

Halley's Theory of the Secular Magnetic Variation

In 1692 Halley, aged thirty-six, extended his theory to explain the secular magnetic variation[24]. He said that his theory of the declination had been well received at home and abroad, but he found two difficulties not easy to surmount. One was that no magnet he had ever seen or heard of had more than two poles, "whereas the earth had visibly four, and perhaps more". The second was that "these poles were not, at least all of them, fixt in the earth, but shifted from place to place"; "whereas it is not known that the poles of a loadstone ever shifted their places in the stone, nor (considering the compact hardness of that substance) can it easily be supposed.

* Hevelius also was interested in the magnetic declination, and in a letter to the Royal Society (*Phil. Trans.*, 5, 2059–2061; 1670) mentions his own observations of the declination at Dansig in 1628, 1642 and 1670.

* Newton's "Principia" contains only five brief references to magnetism; in Axioms, Corollary 6; Book 2, Section 5, Theorem 18; Book 3, Theorem 6, Corollary 5; Theorem 7, Corollary 1; Problem 18, Corollary 10. The third of these references includes the sentence: "The power of magnetism . . . in receding from the magnet decreases not as the square but almost as the cube of the distance, as nearly as I could judge from some crude observations". Newton in 1712 (see ref. 23) made the same remark, when proposing, as president of the Royal Society, that Halley and Hawksbee should make new experiments on the subject.

"These difficulties had wholly made me despond, and I had long since given over an enquiry I had so little hopes of; when in accidental discourse, and least expecting it, I stumbled on the following hypothesis:

"Now considering the structure of our terraqueous globe, it cannot be well supposed that a very great part thereof can move within it, without notably changing its centre of gravity and the equilibre of its parts, which would produce very wonderful effects in changing the axis of diurnal rotation, and occasion strange alteration in the sea's surface, by inundations and recesses thereof, such as history never yet mentioned . . . So that the only way to render this motion intelligible and possible, is, to suppose it to turn about the centre of the globe, having its centre of gravity fixt and immoveable in the same common centre of the earth: and there is yet required that this moving internal substance be loose and detached from the external parts of the earth, whereon we live; for otherwise were it affix'd thereto, the whole must necessarily move together.

"So then the external parts of the globe may well be reckoned as the shell, and the internal as a nucleus or inner globe included within ours, with a fluid medium between. Which having the same common centre and axis of diurnal rotation, may turn about with our earth each 24 hours; only this outer sphere having its turbinating motion some small matter either swifter or slower than the internal ball."

Both the exterior shell and the nucleus were supposed to have their poles distant, by different amounts, from their poles of rotation. The exterior shell, with its magnetic poles fixed, might also have an unequal or irregular distribution of its magnetic matter, explaining the local irregularities in the distribution of the declination. The relative motion of the magnetic nucleus, or, should future observations require it, of the nucleus and additional inner shells, may explain the secular variation, though "it will be very hard to bring this hypothesis to a calculus".

Halley conjectured that the whole period of the secular variation is "700 years, or thereabouts; so that the nice determination of this and of several other particulars in the magnetick system is reserved for remote posterity; all that we can hope to do is to leave behind us observations that may be confided in, and to propose hypotheses which after ages may examine, amend or refute. Only here I must take leave to recommend to all masters of ships and all others, lovers of natural truths, that they use their utmost diligence to make, or procure to be made, observations of these variations in all parts of the world, . . . , and that they please to communicate them to the Royal Society, . . ."

Halley then answers various objections which he foresaw: one was that there is no instance in Nature of the like thing, and that if there were, the nuclei and shells would not preserve the same centre. To this he answers that the ring of Saturn is a notable instance somewhat of this kind. Another objection was that water would leak through the outer shell.

"To those that shall enquire of what use these included globes can be, it must be allowed that they can be of very little service to the inhabitants of this outward globe; nor can the sun be serviceable to them, either with his light or heat. But since it is now taken for granted that the earth is one of the planets, and they all are with reason supposed habitable, though we are not able to define by what sort of animals; and since we see all the parts of the creation abound with animate beings, . . . , all whose ways of living would be to us incredible did not daily experience teach us. Why then should we think it strange that the prodigious mass of matter, whereof this globe does consist, should be capable of some other improvement than barely to serve to support its surface? Why may we not rather suppose that the exceeding small quantity of solid matter in respect of the fluid ether, is so disposed by the Almighty Wisdom as to yield as great a surface for the use of living creatures as can consist with the conveniency and security of the whole. We ourselves, in cities where we are pressed for room, commonly build many stories one over the other, and thereby accommodate a much greater multitude of inhabitants."

Lastly, he explains a diagram showing a nucleus and three shells, the nucleus being about 2,000 miles in diameter, and the thickness of each shell, and of each hollow between, about 500 miles, so that the four upper surfaces are "nearly proportionable to the magnitudes of the planets" Mercury, Mars, Venus and, of course, the earth.

"Thus I have shewed a possibility of a much more ample creation, than has hitherto been imagined; and if this seems strange to those that are unacquainted with the magnetical system, it is hoped that all such will endeavour first to inform themselves of the matter of fact, and then try if they can find out a more simple hypothesis, at least a less absurd, even in their own opinions. And whereas I have adventured to make these subterraneous orbs capable of being inhabited, 'twas done designedly for the sake of those who will be apt to ask *cui bono* and with whom arguments drawn from *final causes* prevail much."

Comments on Halley's Magnetic Theories

How are we to regard this remarkable theory? To us, and probably to some of his contemporary critics, it has some flavour of Kepler in his wilder moments. Halley himself said: "If I shall seem to advance any thing extravagant or romantick, the reader is desired to suspend his censure, till he have considered the force and number of the many arguments which concurr to make good so new and so bold a supposition". A sign that Halley always remained satisfied with these arguments is that a portrait painted in 1736, when he was eighty, shows him holding his 1692 diagram of the earth's nucleus and its shells.

The force and ingenuity of his arguments cannot be gainsaid, and they must be judged by the standards of knowledge and ignorance at the time they were written. But our knowledge of geophysics, especially of seismology, now precludes Halley's hypothesis.

What is more pertinent, however, is that Halley might have found for himself, by experiments like those of Petrus Peregrinus and Gilbert, using loadstones in the form of a sphere and spherical shells, that his hypothesis would not work. The theory of uniformly magnetized spherical shells still lay far in the future, but this experiment was quite open to Halley, and would have shown him that despite the differing directions of the magnetic axes of the nucleus and shells, the field at the outer surface would have only two poles.

We know now that despite the irregular distribution of the earth's field, there are only two magnetic

poles*, to which converge all the magnetic meridians (drawn as Gilbert did on his terrella). Halley's own later knowledge would have enabled him to draw many of these meridians, and to discover this fact. The first actual chart of the earth's magnetic meridians was made in 1817 by another Englishman, Thomas Yeates[24].

The irregularities of the earth's field imply a more detailed irregularity in the inner causes than Halley's hypothesis suggested, and the secular variations are regional in character, not so regular and world-wide as he supposed.

The causes of the main field, of its irregularities, and of its secular variation, are still unknown. It seems likely that the secular variation is connected with convective motions inside the earth, the substance of which becomes first plastic, and then liquid, with increasing depth.

Halley's Magnetic Voyages and Charts

Halley's magnetic theories extended and maintained interest in magnetic observation. Already in 1692 Halley was associated[25] with a plea by Benjamin Middleton, a fellow of the Royal Society, for the Society's assistance "to procure for him a small vessell of about 60 tuns to be fitted out by the Government, but to be victualled and manned at his own proper charges. And this in order to compass the globe, to make observations on the magneticall needle, &c. The President in the name of the Society promised to use his endeavours towards the obtaining such a vessell".

This plan came to nothing, but in 1698 Halley was granted a vessel, the *Paramour* pink, by King William III; "to improve the knowledge of the longitude and the variations of the compasse". Moreover, this landsman of forty-two was put in command of the vessel, with a captain's commission in the Royal Navy—a remarkable event which his deeds well justified. He sailed in November 1698, but trouble with an insubordinate lieutenant caused him to return from the West Indies the following summer, without having crossed the equator. He set off again in September 1699, and after a magnetic survey of the North and South Atlantic down to a high southern latitude he returned in August 1700[26]. In 1701 he published his first chart of the declination over the Atlantic Ocean. In 1702, having collected further observations by mariners in other oceans, he produced a world magnetic chart[27].

These famous charts are Halley's greatest contribution to geomagnetism; they give the declination in a way[28] very convenient to the navigators for whom they were made, and by whom they were widely used for forty years. In time, as Halley foretold, their practical value came to an end, owing to the secular magnetic variation, and new charts replaced them. But the value of Halley's observations as a record of the declination at the epoch 1700 remains imperishable.

One may lament, however, an astonishing gap in Halley's geomagnetic work, his apparent total neglect of the magnetic dip—a fact most surprising in view of his great geometrical knowledge and insight, and of the importance given by Gilbert to the dip in his demonstration that the earth is a great magnet. The dip circle is a more difficult and less accurate instrument than the compass, but its reference direction, the horizontal, is much more easily obtained than the meridian needed in observing the declination. Had Halley observed the dip as well as the declination our debt to him would have been doubled; but where so much is owed, gratitude and admiration must far outweigh these vain regrets.

* Apart from a few additional adjacent pairs of poles, of purely local significance, due to strong local magnetic deposits.

Halley's Later Geomagnetic Work

At least as late as 1721, Halley's published papers manifest his continued interest in geomagnetism. He became the channel by which many observers sent their magnetic measurements to the Royal Society. He likewise presented and discussed many series of observations of two great auroras visible in London, in March 1716 and November 1719. He acutely recognized some of the relations between the auroras and the geomagnetic field, both as regards the location of auroras in high magnetic latitudes and the parallelism of their rays with the magnetic dip needle.

Halley seems to have contemplated further magnetic voyages, because in 1705 Leibniz[29], replying to an astronomical inquiry from Halley, who had then become Savilian professor of geometry at Oxford, genially expressed the hope that younger men would be found to take over the further little excursions of a few thousand leagues which Halley had been considering, to complete his solution of the great magnetic enigma.

But for two centuries Halley had no comparable successor in this field, and the magnetic survey of the globe was not renewed with his abounding zeal until in 1905 the young American, Louis Bauer, with the backing of a prince of industry, the one-time poor Scots lad Andrew Carnegie, resumed the Sisyphean task.

A non-magnetic ship was built for the ocean magnetic survey: unfortunately this was lost by fire in 1929. Later the British Admiralty built another non-magnetic ship, the *Research*, which should by now have completed its first voyage had war not intervened. We may hope that when peace returns the great work which Halley so well began will be taken up again with his own vigour.

[1] NATURE, **150**, 249–257 (1942).

[2] See ref. 22, pp. 203–206; an English version, first published in 1755, is there reproduced with some comments; see pp. 207, 208.

[3] See ref. 22, pp. 203, 87.

[4] English translations of these two poems are given in *Occasional Notes*, No. 9 (1941), of the Royal Astronomical Society.

[5] Mitchell, A. Crichton, *Terr. Mag.*, (*a*) **37**, 105–146 (1932); (*b*) **42**, 241–280 (1937); (*c*) **44**, 77–80 (1939). See also "Geomagnetism", S. Chapman and J. Bartels (Oxford, 1940), chapter 26.

[6] Hellmann, G., (*a*) *Z. Ges. Erdkunde*, **32**, 112–136 (1897) and *Terr. Mag.*, **4**, 73–86 (1899); (*b*) *Neudrucke von Schriften und Karten über Meteorologie und Erdmagnetismus* (Berlin, 1895–97), Nos. 4, 9, 10; (*c*) "Magnetische Kartographie in historischkritischer Darstellung", *Veröff. Preuss. Met. Inst.*, No. 215 (1909).

[7] Thompson, S. P., "Notes on the *De Magnete* of Dr. William Gilbert", English edition for Gilbert Club (London, 1900).

[8] See refs. 5*b*, § 6; 6*c*, No. 10; 7, p. 11; and *Terr. Mag.*, **48**, 6 (1943).

[9] See "Geomagnetism", S. Chapman and J. Bartels (Oxford, 1940), p. 31.

[10] See refs. 6*a*; 5*b*, § 9.

[11] See refs. 6*a*; 5*b*, § 7.

[12] See ref. 5*b*, §§ 10–25.

[13] Refs. 6*a*; 6*b*, No. 10; also *Terr. Mag.*, **48**, 80 (1943).

[14] Ref. 6*b*; see *Terr. Mag.*, **4**, 80, 86 (1899).

[15] Refs. 6*a*; 6*b*, No. 10; 5*c*; *Terr. Mag.*, **48** (1943).

[16] Refs. 6*b*, No. 10; 5*c* (see Note 5); 7, p. 11.

[17] Ref. 7, pp. 59, 60.

[18] Athanasius Kircher, "Magnes sive de arte magnetica" (2nd ed., 1643), p. 568.

[19] Blundeville, "Theoriques of the Seven Planets" (London, 1602); ref. 7, p. 63.

[20] Ref. 6*b*, No. 9.

[21] Ref. 6*b*, No. 10, chapter 3.

[11] MacPike, E. F., "Correspondence and Papers of Edmond Halley" (Oxford, 1932; London, 1937).
[12] MacPike, E. F., "Hevelius, Flamsteed and Halley" (London, 1937); see also MacPike, E. F., "Dr. Edmond Halley, a Bibliographical Guide" (London: Taylor and Francis, 1939).
[13] Or possibly in 1657; see ref. 23, p. 34.
[14] *Phil. Trans.*, 208 (1683).
[15] *Phil. Trans.*, 563 (1692).
[16] Journal Book of the Royal Society; see *Terr. Mag.*, **18**, 118–121 (1913).
[17] See ref. 22, p. 97.
[18] Des Cartes, R., "Principia Philosophiæ" (Amsterdam, 1644).
[19] Bond, Henry, "The Longitude Found" (London, 1676).
[20] Ref. 25, § 9.
[21] Ref. 25; here abridged.
[22] See ref. 22, pp. 135–137; these experiments are discussed in detail by A. C. Williams, "Edmond Halley and the Problems of Terrestrial Magnetism" (London University Dissertation, 1937).
[23] Copies of Yeates' chart are in the British Museum Library (Catalogue No. 974 (2)) and the Admiralty Library.
[24] See ref. 22, p. 186 (Hooke's notes for Jan. 11, 1692/3 and April 12, 1693), also footnote 2.
[25] See ref. 22, pp. 8, 9, 21, 22, 103–115, 243–247, for much interesting information on Halley's magnetic voyages.
[26] See ref. 4.
[27] The method had previously been used by Christopher Burro, who made an isogonic chart mentioned by Kircher (*loc. cit.*, p. 443).
[28] See ref. 22, p. 201, and ref. 23, p. 60.

MORE AND BETTER MILK

IT is common knowledge that while the average diet in most European countries has been seriously worsened by the War (in certain countries it is reported to have been deliberately reduced by the Nazis well below the minimum subsistence-level), the present diet in Great Britain, though drab and much restricted as to choice of food, is still from the point of view of the nutritional scientist little, if at all, inferior to pre-war standards. In fact, as a result of the rationing system and the more even distribution of purchasing power, it is probably true that the great majority of our population is better (or perhaps one ought to say less badly) fed than before the War.

That this is so is due in no small part to the facts that it has been possible (*a*) to maintain the *total* home production of milk at its pre-war level; (*b*) to increase considerably the quantity of ***liquid*** milk consumed (at the expense of milk which was, in pre-war days, manufactured into products valuable and delectable in themselves but the preparation of which entailed very considerable losses of nutritive value in the shape of whey and skimmed milk, little of which was consumed by the human population); and (*c*) to increase slightly the amount of liquid milk produced in winter when milk is more difficult and economically more costly to produce than in summer. As regards (*b*), during the last three years the yearly liquid milk intake in Britain has risen until it is now about 40 per cent above pre-war level. This is a continuation of a tendency that was clearly evident before the outbreak of war. In 1938, for example, the liquid milk consumption per head in England and Wales rose by 11 per cent.

For the first time in our history a comprehensive Government milk policy, in which the Ministries of Food, Health and Agriculture are collaborating, is now in action, based on the key nutritional importance of milk in the war-time dietary. A number of different but associated schemes have been introduced to implement that policy. They include on the consumption side an extension of the pre-war milk-in-schools scheme, the legislation for giving priority of liquid milk supplies at reduced cost to expectant and nursing mothers and children less than five years of age and to certain classes of invalids, and on the production side the priority given to dairy cows in the animal feeding stuffs rationing scheme, the nation-wide drive through the County War Agricultural Executive Committees to stimulate milk production, the voluntary 'panel' scheme arranged a few months ago between the Ministry of Agriculture, the National Farmers' Union and the National Veterinary Medical Association for the control of certain diseases affecting dairy cows, the national milk testing and advisory scheme to prevent milk souring and to disseminate knowledge of methods of clean milk production on the farm and of hygienic milk handling off the farm, together with national schemes for milk recording and live-stock improvement.

A joint meeting of the Nutrition Society and the Food Group of the Society of Chemical Industry took place at the London School of Hygiene on July 3 to review the nutritional aspects of these and other recent developments and trends in milk policy. The chairman (Prof. H. D. Kay), in his introductory remarks, pointed out that the future of national nutrition is bound up to a great extent with milk production, and that the future of milk production is in a very real way the future of British farming. He emphasized the present difficulties under which milk producers were working, and pointed out the urgent need for improved technical training both for the future dairy farmers and for those entering the technical and scientific side of the milk processing and distribution industry. For the latter entrants there was at present, as before the War, scarcely any advanced vocational training in Great Britain.

The first paper was given by Mr. J. L. Davies, on the production, supply and marketing of milk. He emphasized the enormous size of the milk industry, with a capital estimated (in pre-war values) at £350–£400 millions, and with between 400,000 and 500,000 people actually engaged in milk production, processing and distribution, apart from those in the ancillary industries of feeding stuffs, manufacture and production of dairy machinery, equipment, etc. Of the herds owned by our 150,000 dairy farmers, 36 per cent contained less than 10 cows, 45 per cent 10–25 cows, 15 per cent contained 25–50 cows and only 4 per cent contained more than 50 cows. While the average yield of milk per cow had gone down since the War, he considered that the pre-war average milk yield for the cow in the older established dairying herds was considerably greater than the average for the whole country (which included some herds mainly kept for breeding and rearing purposes), say, 600–650 as against the total country average of 500–550 gallons. There was room for improvement in yield and in methods of milk production, and steps were now being made through W.A.E.C.s to achieve as much as was possible in war-time. Mr. Davies suggested a ten-year plan, to rebuild and re-equip our dairy farms, to bring water supplies and electricity to them, and to render them relatively disease-free. He advocated a six-day (and not, as at present, a seven-day) week on the dairy farm. His ten-year plan would include the steady raising of milk consumption to at least 0·75 pint per head per day accompanied, of course, by the same increase in milk production, which he was sure the industry could provide during the same period. He considered we should plan now.

In the discussion on this paper, Dr. Wooldridge emphasized the importance of disease control on the farm. Disease placed a high and unnecessary tax on the economic cost of milk production. Prof. A. W. Ashby, while supporting the need for greater efficiency in the industry, which could only be achieved by national planning, considered that the economic effect of disease was exaggerated. Dr. Stableforth held the opposite view. Mr. Davies' paper and the discussion which followed took up the whole of the morning session.

In the afternoon, Dr. A. T. R. Mattick dealt with the bacteriological aspects of milk processing. Pasteurization, if properly carried out, both greatly increased the keeping quality of milk and destroyed all the common pathogenic organisms that might occur in raw milk. The difficulty was to ensure that pasteurization was properly carried out. For public health reasons there should be filtration of the milk *before* pasteurization. The provisional regulations for the new attractive alternative of high-temperature short-time pasteurization (not less than 162° F. for not less than fifteen seconds) allowed a margin of safety, but the thermometric apparatus controlling flow diversion valves lacked, in some machines, speed of response to changes in temperature. He discussed recent findings on the occurrence in milk of pathogenic and other organisms. Aseptically drawn milk from healthy animals contained only micrococci and Corynebacterium sp., organisms which did not survive ordinary pasteurization. In fact the organisms in cleanly but not aseptically drawn milk were all vulnerable to pasteurization. This was not the case with raw milk reaching the average pasteurization plant, in which heat-resistant organisms were of frequent occurrence. The bacterial quality of raw milk seriously affected the bacterial quality of the milk after pasteurization. Dr. Mattick very much doubted the value of the plate count as a measure of efficient pasteurization. He considered that there was much room for improvement in the design of bottle-filling and bottle-washing machines. With improved methods of production and control on the farm, coupled with improvements in processing technique and particularly in the manipulation of milk after heat treatment, "we have the possibility of furnishing the people with a product of such superb quality as is at present visualized only by the few".

Dr. S. K. Kon followed with a paper on the chemical composition and nutritive value of milk and milk products. This paper gave a wealth of recent quantitative findings impossible to summarize in a review. Milk varied in chemical composition and nutritive value not only with the breed and health of the cow, but in the same healthy cow from one season of the year to another. The diet of the cow was largely responsible for changes in the vitamin A content of milk and to a lesser extent for the changes in riboflavin; but vitamin B_1, which was synthesized in the bovine rumen, was fairly constant in quantity throughout the year. Vitamin C was synthesized by the cow, but unlike the B vitamins the synthesis did not take place in the rumen. It was unfortunate that milk was lowest in vitamin content in early spring, since most other human foodstuffs were at that season at their nadir of nutritional value. He considered that more attention should be paid in future to nutritive quality of milk produced in Great Britain.

The effect on the nutritive value of milk of pasteurization, sterilization, drying and condensing was stated in some detail by Dr. Kon. Commercial pasteurization brought about a loss of vitamin C of the order of 20 per cent, caused rather by exposure of the milk to light than to heat, and a still smaller percentage loss of vitamin B_1 but no other change of nutritional significance. The effect of sterilization was rather larger—a loss of half the vitamin C and one third of the vitamin B_1. There is, in any event, little vitamin C in cow's milk, and not very much D. But even the small quantity of vitamin C in a pint bottle of raw milk was halved if the bottle was left on the doorstep in full daylight for half an hour; destruction was still more rapid in sunlight. Spray drying of milk led to a very small loss in nutritive value, of the same order as that caused by pasteurization. Roller drying brought about rather larger losses. Vitamin A and carotene, vitamin D and riboflavin suffered no loss during drying. Among other important points Dr. Kon emphasized the nutritional importance of the by-product from butter manufacture—skim milk. Dried skim milk ('National Household Milk') was an excellent food for all ages except early infancy.

The discussion on these two papers was opened by Prof. G. S. Wilson, who stressed the great difficulty that responsible medical men had in advocating the increased consumption of milk unless the latter had previously been efficiently pasteurized. He dealt with the human diseases that might be conveyed by raw milk, and exploded several of the arguments brought forward by the opponents of compulsory heat-treatment of milk. A lively discussion followed, in which Mr. A. L. Bacharach, Dr. H. E. Magee, Dr. N. S. Barron, Dr. W. A. Lethem, Captain H. Barkworth, Prof. J. R. Marrack, Dr. Kent-Jones and others took part. Perhaps too much attention was paid in the discussion to the pasteurization problem—the major facts concerning which are well known. In consequence, some of the important bacteriological and nutritional points made by the principal speakers in the afternoon session were crowded out.

In summing up, Prof. Kay stated that the real war-time problem was to get to every consumer the maximum quantity of safe winter milk, and he emphasized the great contribution that dairy farmers and those advising them and directing their efforts had made in maintaining the national winter milk supply during the War. There seemed to be no dissentients in this representative meeting of nutritionists (nearly three hundred were present during the afternoon session) to the view that general heat treatment of milk was highly desirable, in fact overdue, and that not least from the point of view of the true interests of the producer. It was also clear that the meeting fully agreed with the importance of increased consumption of liquid milk, and that the objective should be an average of not less than 1 pint per head per day. There was general agreement that one of the questions of greatest moment at present from the point of view of national nutrition was how the trend, clearly evidenced during the War, towards increased *per capita* milk consumption was to be maintained in the post-war period until this objective was achieved. Planning for increased post-war production and consumption should begin forthwith. There were three sides at least to this question, the producer's, the distributor's and the consumer's, and better technical training for all three, not excluding the frequently forgotten consumer, was urgently needed if this problem were to be solved in a reasonable time.

SOME CZECHOSLOVAK CONTRIBUTIONS TO CHEMISTRY IN THE NINETEENTH CENTURY

By Dr. GERALD DRUCE

THE modern development of chemistry in Bohemia may be considered to have passed through five phases. These are: (1) an initial era closely associated with the Czech national revival movement and mainly concerned with descriptions and analyses of local natural resources; (2) advancement in teaching contemporary science; (3) improvement in analytical technique and corresponding progress in methods of research; (4) the period of Brauner's atomic weight determinations and researches on the rare earths; and (5) varied research activities between 1918 and 1938.

The latter part of the fourth phase and the whole of the last one, when Czechoslovak chemists made some notable advances, fall outside the scope of the present survey. Similarly, the beginnings of the national revival (in which men of science played a leading part) date back to 1790, or perhaps to 1780, so that only the later, but scientifically more exact, work of the naturalists and chemists of this period will receive notice here.

The establishment of the Bohemian National Museum and the "Museum Society" in 1818 gave an impetus to the study of the sciences. Among the collections of local minerals presented to the Museum were specimens of unknown composition awaiting examination. There were also many economic native and exotic plants in the new herbarium, which soon included many thousands of specimens. Lastly, the medicinal qualities of the thermal and spa waters were then attracting attention and trustworthy analyses were urgently needed.

A typical mineral study was that of J. S. Presl on pribramite, a hydrated silicate of iron containing manganese and magnesium[1]. Many of these mineral analyses were made by men who were primarily geologists, like Prof. F. M. Zippe. Some of the Bohemian aristocracy were also interested in the natural sciences. In 1831, Count B. W. Berchtold analysed the efflorescence observed on bricks and known as "air salt" and referred to in his memoir as "Bruska salt" after the Prague suburb where it was collected[2].

In 1819, J. J. Steinmann prepared strychnine and isolated morphine[3], and a newly formed "Patriotic Economic Association" allocated funds for extracting it from poppies. The same body had also entrusted Prof. Mikan with the task of 'de-sulphurizing' coal. Presl and Steinmann (then professor of chemistry at the University of Prague) examined Kysibl Spa water[4], which later became a famous table water; Dr. A. Jungmann gave a comprehensive account of many other thermal waters[5]. When Berzelius visited Carlsbad in 1831 he examined the gas from the hot springs and estimated that it contained 1 per cent of nitrogen; it is mainly carbon dioxide, but is now known to contain about 1 per cent of inert gases, including radon.

There was a lack of laboratory facilities in Prague during the first quarter of the century and the few Czech chemists of note had little chance of making their work known except through German publications. Their only publications were the proceedings of the Bohemian Scientific Society (founded in 1775), a quarterly publication issued by the Museum, and Presl's periodical, *Krok*, which appeared at irregular intervals between 1821 and 1840. Though primarily a biologist, Presl devised a scheme of dry tests for the qualitative detection of metals, but it was not widely used. He wrote the first Czech text-book of chemistry, the inorganic part appearing in 1828 and the organic part in 1836[6]. It was a useful pioneer treatise that was superseded after 1841 by Prof. Amerling's[7]. This book, which recognized fifty-six elements, traced the development of crystallography and touched upon biochemistry. Thus, Amerling pointed to the constant occurrence of certain metals (potassium, calcium, magnesium and iron) and other elements (chlorine and sulphur) in plants and animals. He also appreciated the value of Faraday's work, and in surveying earlier developments he emphasized the importance of Black's discoveries with the alkaline earth carbonates.

These text-books gave Czech students a knowledge of chemistry in their own tongue. Previous lecturers like Mikan, Freysmuth, Neumann (Brauner's grandfather and author of a German "Lehrbuch der Chemie", 1810) and Balling (who studied both fermentation and metallurgy) had used German works. Noteworthy among these students was B. B. Quadrat, who, in 1844, investigated the double platinum cyanides[8] and prepared phenyl thiocyanate and studied its reactions and decomposition products[9]. Further contributions to organic chemistry were made at this time by Prof. J. Lerch, who isolated a number of monobasic acids (caproic, caprylic, capric, etc.) from various animal fats[10]. He also tried to elucidate the nature of the explosive, potassium carbonyl, sometimes formed during the preparation of potassium by dry distillation of the tartrate.

The outstanding figure of the third era of Czech chemistry was František Štolba (1839–1910), professor of chemistry at the Prague Polytechnic. He has been styled "the Czech Bunsen" on account of his influence and his extensive researches, though these never reached the magnitude or importance of those of the German chemist. From 1865 onwards Štolba published several hundred original papers, most of which were abstracted for the *Journal of the Chemical Society*, while the *Chemical News* referred to more than fifty of them. In 1877 Štolba founded the *Chemické Listy*, thus giving Czechoslovak chemists the opportunity to announce their discoveries in their own language.

In Štolba's first communication he demonstrated that both water and hydrochloric acid have an appreciable action on lead[11]. He proceeded to many analyses of local interest. Thus, he showed that the organic matter in water from Bohemian rivers was related to its calcium content. Another observation typical of the man was that platinum vessels lost in weight when heated in coal gas flames, though polished platinum lost less than unpolished. This was borne in mind later by Brauner, who not only kept his platinum ware highly polished but also always heated it inside another vessel. Štolba was one of the first to extract indium from zinc blende and tellurium from lead chamber mud, while another observation related to the identity of potassium chlorate crystals obtained either by fusion or by crystallization from aqueous solution. But his most important contribution to chemistry was his researches in the silico-fluorides.

Štolba prepared quantities of hydrofluoric and hydrofluosilicic acids and showed that the latter

dissolve metals, for example, iron, evolving hydrogen. With this acid he made silicofluorides of cerium (from its acetate), cobalt (from the carbonate), ammonium (from the iron salt and ammonium chloride) and calcium (by heating together fluorspar, infusorial earth and hydrochloric acid). Štolba also noticed that beads of potassium silicofluoride heated in a blowpipe flame on platinum lost silicon fluoride and left a mixture of potassium fluoride and silicate. Curiously enough, Štolba's method for the analysis of silicate minerals began with sodium carbonate fusion, followed by re-fusion under salt. He left it to his colleague, the mineralogist Prof. E. Bořický, to 'open up' these minerals with hydrofluoric acid.

No account of Štolba's work would be complete without reference to his monograph advocating wider laboratory use of nickel. He emphasized its suitability for making crucibles, spatulas, tweezers, etc., and he indicated the precautions to be observed. He was also alive to the possibilities of aluminium, which was just becoming available in the 'eighties.

Among other noteworthy chemical discoveries made by Czech chemists in the middle of the last century are those of Prof. Karel Preis, lecturer at Prague Polytechnic and later founder of the Sugar Research Institute. Before devoting himself to technological researches he studied the action of iodine on unsaturated organic compounds, made the bromides of tin and some double salts in a pure state and came across a curious double sulphide of iron and potassium[13]. With Dr. B. Rayman he oxidized cholesterol and some derivatives with nitric acid. That more Czechs did not interest themselves in organic chemistry at this time is difficult to explain. Prof. A. Bělohoubek, contemporary with Štolba, showed that pure methyl alcohol never gave chloroform with bleaching powder. From a comprehensive survey of organic compounds he concluded that the richer these are in oxygen the more soluble they are, while liquids are mutually soluble the more similar their structure.

During the last quarter of the century Czech inorganic chemists made good progress. It was dominated by the work of Brauner and his collaborators, which overshadowed the less spectacular but still important contributions made by Wald and Stoklasa.

Prof. F. Wald was a physical chemist who maintained that stoichiometrical laws are deducible without recourse to any atomic hypothesis. In 1887, independently of J. W. Gibbs, he propounded a theory of phases on the basis that all substances that mix continuously and homogeneously constitute a single phase. Therefore, gases exist as one phase only, since two or more gases or vapours never form sharp boundaries. Liquid phases are more numerous, while with solids it is still more difficult to characterize one or more phases. It would be difficult to summarize Wald's views in a few lines[13], but his definition of an element may be taken as typical:

"Amongst chemical individuals there is a group of substances which possess a remarkable property. From them may be prepared by chemical reactions other pure substances, yet they are never alone a product of other reactions. Such bodies are called elements. If reactions which are described in a three-membered equation did not exist, elements would remain unknown to us; no substance of a reaction, in which a certain number of reactants gives the same number of resultants, can be regarded as a part of the others. Our knowledge as to the number of elements remains, however, provisional as we have no guarantee that the substances now regarded as elements will not be shown to be decomposable and interchangeable."

Ostwald, in his Faraday Lecture in 1904, referred to Wald's views in the following words:

"The question whether it is possible to deduce the stoichiometrical laws without the help of the atomic hypothesis has only been raised by other investigators in order to deny the possibility. So far as I am aware there exists only one man (Wald) who has worked on the question with the earnest hope of obtaining an affirmative answer . . . to him I owe first the idea that the definition of substances and elements is in a certain sense arbitrary, although very helpful and convenient. This definition is a condensed expression of our methods of separating and purifying these bodies. While, generally speaking, every solution has the same claim to be investigated as these bodies, the latter soon distinguish themselves as standards to which all other cases may be referred. To Wald I owe further the idea that the conception of a phase is a far more general one than that of a substance, and that the deduction of the laws governing the nature of substances must start from the conception of a phase. . . ."[14]

Stoklasa's researches were partly analytical and partly biochemical. Among other matters he showed that both iron and phosphorus are essential elements for the formation of chlorophyll in plants. In his lengthy study of lecithin he found that it decomposes in the absence of phosphates and that plants lacking phosphates retrogress for this reason.

Brauner's work, lengthy and important though it was, can only receive very brief notice here. It began in 1877 with his observations on beryllium and had for its main object the substantiation of Mendeléeff's Periodic Law[15], the great value of which Brauner was one of the first to appreciate. Most of his atomic weight determinations and his separations of the rare earth elements were made before 1900, and indeed Mendeléeff stressed their importance so early as 1889 in his Faraday Lecture[16]:

"The Periodic Law, as Brauner repeatedly insisted, was against the formula, Be_2O_2. It required the formula BeO, *i.e.* an atomic weight of 9. . . . I consider the confirmation of Brauner's view as important in the history of the law as the discovery of scandium."

Science is international in its ramifications, and the discoveries of savants of one nation influence and inspire the work of others in distant lands. Not only did Brauner make contact with Russian men of science, but he also established connexions with British and American chemists; and at the close of the nineteenth century many young Czech chemists, beginning their careers, spent part of their student or post-graduate years abroad. At this time, Štěrba-Böhm was collaborating with Brauner in further studies of the rare earths, while Votoček had begun his life-work on the less common sugars. After closely studying arabinose he examined rhodeose, a methylpentose that he had isolated from the glycoside convolvulin[17].

In chemistry, as in other sciences, Czechoslovaks made notable advances during the nineteenth century. This work continued to expand and extend until 1939, and when they are able to resume their work for the

progress of mankind it can be confidently predicted that they will contribute again to the advancement of chemistry.

[1] *Krok*, 2, 63 (1824).
[2] *Krok*, 2, 616 (1824).
[3] *Schweigg. J.*, **25**, 410 (1819).
[4] *Časopis Českého Musea*, **5**, 297 (1831).
[5] *Časopis Českého Musea*, **5**, 80 (1931).
[6] "Lučba čili Chemie Zkusná" (Prague, 1828 and 1836).
[7] "Přehled Lučby čili Hmotozpytu" (Prague, 1841).
[8] *Časopis Českého Musea*, **22**, 143 (1848). *Ann.*, **63**, 164 (1847); **65**, 249 (1848).
[9] *Ann.*, **71**, 13 (1849).
[10] *Ann.*, **49**, 221 and **49**, 212 (1844); **59**, 57 (1846).
[11] *Chem. News*, **11**, 226 (1865). The General Index to the first 100 volumes of the *Chemical News*, and the Decennial Indexes of the *Journal of the Chemical Society* give many references to Štolba's papers and to those of other Czech researchers.
[12] *Chem. News*, **20**, 70 (1869); **36**, 90 (1877) and **50**, 104 (1885).
[13] An account in English of Wald's theory is given in *Collection*, **3**, 9 (1931).
[14] *J. Chem. Soc.*, **85**, 506 (1904).
[15] See NATURE, **150**, 632 (1942).
[16] *J. Chem. Soc.*, **55**, 634 (1889).
[17] *Vestník Král. Česk. Spol. Naук.* (1899).

OBITUARIES

Dr. C. J. S. Thompson, M.B.E.

DR. C. J. S. THOMPSON, who died recently, was born at Liverpool in 1862. As a young man he studied chemistry and pharmacy and became interested in the historical aspect of these subjects. In 1898 his close association with what was ultimately to become the Wellcome Historical Medical Museum commenced. From this date he played an active part in assisting Sir Henry Wellcome in the work of collecting, classifying and assembling the ever-increasing store of material. In 1913, he became the first conservator of the Wellcome Historical Medical Museum.

Thompson travelled widely and was a successful collector: he was much interested in the literature of his subject, and played an important part in establishing the large research library attached to the Museum. In the International Congresses of the History of Medicine he played a prominent part, more especially in the Congress held in London in 1922. He was a prolific writer with a wide knowledge of his subject, a ready pen and a popular style. He covered a large field in his numerous articles and books, many of which were popular in character and dealt with such subjects as alchemy, crime, quackery, apparitions, monsters, magic and love. He also compiled in collaboration with Sir D'Arcy Power a most useful "Chronologia Medica".

His connexion with the Wellcome Museum terminated in 1926, and in the following year he was elected honorary curator of the Historical Collection of the Royal College of Surgeons. His "History and Evolution of Surgical Instruments", published in the United States in 1942, was opportune, for in May 1941 much of the collection had been destroyed by enemy action.

During the War of 1914–18 he was in charge of an auxiliary military hospital at Harrow, for which work he was awarded the M.B.E.

Thompson was a great enthusiast, an indefatigable worker, and a good organizer. For such a man retirement is merely the opportunity for further work and, to the end, he was busily occupied with the subject which he had made so peculiarly his own. He did much to popularize a branch of medicine which in Great Britain has never received the recognition it deserves. S. H. DAUKES.

Dr. R. W. Gray

ROBERT WALKER GRAY, born at Peterhead, Aberdeenshire, in 1864, came of whaling stock. His grandfather and his father were well-known skippers in the Peterhead whaling fleet, and the latter, doubtless inspired by the example of the Scoresbys, made many records of his observations in the Arctic seas. David Gray's "Notes on the Greenland Whale" appeared in annual reports of the Scottish Fishery Board and elsewhere, accounts of the year's whaling in the *Buchan Observer*, and Southwell made use of some of his information in his articles on sealing and whaling. It was natural, therefore, that one of the sustained interests of the son, R. W. Gray, should be in whales and whaling. His name appears in the roll-book of the natural history class at the University of Edinburgh for the session 1887–88, when he was in his twenty-third year, and he graduated in medicine at that University in 1892. Having already made several voyages to the Greenland Sea, he became surgeon upon his father's whaler, *Eclipse*, at a time when the Peterhead whaling was all but extinguished, and his subsequent experience included voyages with the Orient Line as medical officer, the study of sleeping sickness in Africa, and a long period of general practice in towns on the south coast of England.

Since his earliest accounts of voyages to the Greenland Sea appeared in the *Zoologist* in 1887 and 1889, Dr. Gray continued to publish articles bearing upon the history of the whale-fishery and on whales and their habits. He contributed the account of "Whale Fisheries" to the ninth edition of the "Encyclopædia Britannica", and his description of the rise and decline of the "Peterhead Sealers and Whalers" in the *Scottish Naturalist* of 1932 and 1933 was a valuable contribution to the history of the whaling industry. Dr. Gray's knowledge of the literature of whaling was thorough, and he made good use of it and of his own experiences in discussing, in the *Naturalist* and in occasional letters to NATURE, debated points concerning the physiology of whales.

Dr. Gray had a quiet unassuming manner and a mind richly stored with memories of his adventures in the Greenland Sea and Davis Straits. Having retired from medical practice, he died early in July in Exeter, after a short period of failing health, in his seventy-ninth year. JAMES RITCHIE.

WE regret to announce the following deaths:

Dr. H. C. Bumpus, director of the American Museum of Natural History, New York, during 1902–11, on June 21, aged eighty-one.

Mr. F. J. Cleminson, honorary director of research, Ferens Institute of Oto-laryngology, Middlesex Hospital Medical School, on August 21, aged sixty-five.

Mr. Arthur S. Newman, the well-known camera designer and photographic expert, on August 12, aged eighty-two.

Prof. F. Schlesinger, emeritus professor of astronomy in Yale University, on July 10, aged seventy-two.

NEWS AND VIEWS

Carnegie United Kingdom Trust

THE twenty-ninth annual report of the Carnegie United Kingdom Trust covering the year 1942 shows that the operations of the Trust have been more concerned with the development or the completion of old schemes than with the initiation of new ones. In regard to youth services, the system of club equipment grants has been continued in England, Wales and Scotland, and has also been extended to Northern Ireland. The year's payments amount to £5,695 and substantial sums are still outstanding in respect of promises not yet taken up in full; the 1942 promises at December 31 stood at £12,168. Shortage of equipment due to the supplies situation and the scarcity of trained leaders are likely to present serious difficulties to any large expansion of the youth services at present. The limited revival of the former club library policy has met with very encouraging responses, and grants in aid of the headquarters administration of the national associations of boys' clubs, girls' clubs and young farmers' clubs were maintained in 1942 at the same rate as in the previous year. Notice of the discontinuance of these grants has now been given, but further assistance has been promised to the Eighteen Plus movement during the three years 1943–45. Grants for the training of leaders, of £1,200 to the National Association of Girls' Clubs for local supervisors of training and £1,000 for bursaries, were continued from 1941. Grants were also made during 1942 in aid of the administration of the Land Settlement Association, the Museums Association, the National Council of Social Service and the Rural Development Council of Northern Ireland, and towards the maintenance of the three central libraries. In regard to music and drama, the work of the Musical Education Committee has continued on the same plan as in 1941 but on a larger scale. The work of the Joint Committee for Drama also showed an increase in expenditure, and the report comments on the remarkable way in which musical and dramatic activities are not merely surviving the War but even extending and fortifying themselves.

Utilization Investigations of Forest Products

As during the War of 1914–18 so in the present War, the utilization of forest products has assumed a first-class importance in India. During the War of 1914–18 the exports from the Indian forests, mainly timber and forage, went chiefly to the Middle East. But India herself had to become self-supporting in several commodities such as, for example, soap, matches, bobbins for weaving and even to a greater extent railway sleepers, when these products could no longer reach her by import. This state of affairs has repeated itself during the present War, but has become more exacting through the greater demand on India to provide war supplies for her own needs and including those of a rapidly expanding modern army. This position fully explains the reason for the inevitable expansion of the work of the Utilization Branch of the Dehra Dun Forest Research Institute. Research on timber-seasoning kilns has already been dealt with (see NATURE, July 25, 1942, p. 127). Indian Forest Leaflet No. 34, Utilization (1943), "Types of Timber Seasoning Kilns suitable for drying Indian Woods", revises Leaflet No. 11, issued on the subject. Leaflet No. 33 (1943) deals with the detailed design of timber roof trusses made with disk dowel joints. Timber structures fitted in this fashion have not previously been used in India. The data incorporated in the leaflet are the result of actual tests carried out in the laboratories of the Institute.

Indian Forest Bulletins, Utilization, Nos. 114 and 117 (1943) discuss the "Treatment of Green Hollong (*Dipterocarpus macrocarpus*) Sleepers" and the "Testing of and Improvement in Design of Packing Cases". The green hollong sleepers were treated with a mixture of creosote and fuel oil; by conditioning the sleepers by steaming and vacuum or the Boulton process, or a combination of both, they could be satisfactorily treated, on an average 30 per cent moisture being lost. The packing case investigation was carried out at the instance of the Ordnance Stores Department. Cases for packing medical stores were the ones to which the investigation was confined. Three types of cases were examined, and the improvements proposed are discussed in the bulletin.

Our Knowledge of the Physical World

DR. A. C. EWING, in an article entitled "Knowledge of Physical Objects", has discussed the question, "How can we justify the claim to know the physical world?" (*Mind*, April 1943). He first considers the contention that it is meaningless to entertain a *general* doubt about such knowledge because there are no possible experiences which could cast doubt on *all* physical propositions, only on some. He answers it by pointing out that dreams and illusions are the experiences on which the general doubt is based, and even though they are not conclusive evidence, the doubt has meaning by reference to them. This was a point well worth making. He next examines Prof. Moore's view that we can know with certainty something which it is both logically possible might not be true and also logically possible we might be wrong in thinking we knew. Our knowledge of physical objects is of this kind, according to Moore, and we know them without knowing how we know them. As Moore does not hold that we know physical objects directly, it seems possible to raise a doubt about our knowledge by pointing out that induction, by means of which this knowledge is established, cannot yield certainty. Briefly, Moore's answer to this is that the proposition that inductive argument cannot yield certainty is itself *less* certain than the proposition that he knows there is a chair.

The author develops his own position from this point by urging that strictly we can only speak of knowing that S is P where "S is P" is *objectively* as well as subjectively certain; but that we commonly say we 'know' that S is P where we are practically certain, though error is theoretically possible. In the strict sense we do not know that there are physical objects; in the other sense we do. Such knowledge is based on induction and therefore it must be admitted that any particular proposition which we know can by further evidence be rendered more certain than it is at any particular time. This is the Achilles' heel of the author's view. The present writer cannot believe that the existence of the table on which he wrote these notes, and on which a short time ago his supper was laid, can be rendered more certain. The distinction drawn by Dr. Ewing does not solve the problem. It simply takes us back to the discarded view that the existence of physical objects cannot be known, though it can be rendered highly probable.

Vital Statistics of Scotland

ACCORDING to the quarterly return of the Registrar-General of Scotland, 1942 was a very healthy year, as is shown by the fact that the death-rate from all causes was 13·0 per 1,000 of the population, or 1·5 below the corresponding rate for 1941 and 0·7 below the average for the quinquennium 1936–41. The birth-rate was 18·1 per 1,000, or 0·2 above the average for 1941 and 0·5 above the five-year period. Both infantile mortality and maternal mortality showed very satisfactory rates, the former being 69 per 1,000 live births and the latter 4·0 per 1,000 total births, which was the lowest rate on record for about four decades. The deaths from the principal epidemic diseases were the lowest on record, amounting to 1,060. As regards the chief chronic diseases with a high mortality, the death-rate from all forms of tuberculosis was 80 per 100,000. Though there was a decline in non-respiratory tuberculosis, especially in the age-group fifteen to twenty-five, there was a definite rise in the respiratory tuberculosis death-rate. Increase in the death-rate for syphilis was shown mainly by an increase in aneurysm and congenital syphilis. The death-rate from malignant disease was 171 per 100,000, which represented an increase above the 1941 rate. On the other hand, there was a definite decline in the death-rate for cardiac and respiratory diseases apart from tuberculosis and malignant disease.

Syphilis in the United States

IN a recent paper (*J. Amer. Med. Assoc.*, **122**, 365; 1943) Dr. W. G. Smillie maintains that the incidence of syphilis among white men in the greater part of the United States is low, and that it is limited for the most part to the lowest classes of society, so that it has become a disease largely of the ignorant, the careless, the criminal and the social outcast. Syphilis, however, is more than ten times as prevalent among the Negroes as among white persons. In recent examinations for army service, the incidence of syphilis among whites was 0·2 per cent, as compared with 11·2 per cent among Negroes.

Tuberculosis in Buenos Aires

ACCORDING to Dr. Alejandro A. Raimondi (*J. Amer. Med. Assoc.*, **208**, 121; 1943), director of the Municipal Centre of Buenos Aires for a Campaign against Tuberculosis, there were 2,708 deaths from that disease in Buenos Aires in 1941. The increase in the disease is due to the influx of tuberculous subjects from the provinces. The routine practice of taking skiagrams of children has resulted in finding tuberculosis where it was not suspected and also in preventing the spread of the disease. X-ray examination of the chest of apparently normal persons has yielded good results. Tuberculosis has been found in only five cases (1·3 per cent) in a group of 384 students and in only twenty-five cases (3·02 per cent) in 826 applicants for municipal posts.

Agricultural Research Council Appointments

THE Committee of the Privy Council for Agricultural Research has appointed the Right Hon. the Earl De La Warr to be chairman of the Agricultural Research Council in succession to the late Sir Thomas Middleton. The Committee has further appointed Prof. I. de Burgh Daly to be a member of the Agricultural Research Council in succession to Sir Joseph Barcroft, whose term of office as a member has expired.

Library Stocks of Periodicals

REFERRING to the letter on "Library Stocks of Periodicals" published in NATURE of July 24, Mr. C. C. Barnard writes: "I inadvertently failed to appreciate the proposals of a committee of the Association of Special Libraries and Information Bureaux, which has under consideration the preparation of a Union Catalogue of Periodicals in British Libraries. This Committee proposes at present to limit its plan to the compilation of a consolidated check list composed of entries in the 'World List of Scientific Periodicals', the 'Union Catalogue of Periodical Publications in University Libraries', and the relevant section of the 'British Museum Catalogue'. The work which it contemplates should be of assistance in the accomplishment of the larger scheme which I outlined."

Wellcome Junior Fellowship

THE Joint Advisory Committee of the Wellcome Foundation and the Veterinary Educational Trust has awarded the first Wellcome Junior Fellowship to Mr. John Lochiel McGirr. The fellowship is tenable for one year at £400 per annum and is subject to renewal for a further period of one or two years at the discretion of the Advisory Committee. Another fellowship of equal value will be awarded in April 1944. The award to Mr. McGirr represents the first result of the recent foundation of the Veterinary Educational Trust.

The Night Sky in September

FULL moon occurs on September 14d. 03h. 40m. U.T. and new moon on September 29d. 11h. 29m. The following conjunctions with the moon take place: Sept. 2d. 00h., Mercury 7° S.; Sept. 20d. 03h., Mars 4° N.; Sept. 21d. 05h., Saturn 3° N.; Sept. 25d. 14h., Jupiter 0·3° N.; Sept. 26d. 16h., Venus 8° S. The following occultations of stars brighter than magnitude 6 take place: Sept. 8d. 21h. 02.4m., 15 Sgr (*D*); Sept. 10d. 21h. 27.3m., σ Cap (*D*); Sept. 26d. 11h. 08.2m., α Leo (*D*); Sept. 26d. 12h. 12.1m., α Leo (*R*). The times are given for Greenwich and *D* and *R* refer to disappearance and reappearance respectively. Mercury, an evening star at the beginning of the month, sets a quarter of an hour after the sun on Sept. 1. The planet is approaching the sun and is in inferior conjunction on Sept. 24. Venus is in inferior conjunction with the sun on Sept. 6. At the middle and end of the month the planet can be seen as a morning star, rising at 4h. 43m. and 3h. 20m. respectively. Mars, in the constellation of Taurus, is an evening star and sets at 13h. 20m., 13h. and 12h. 32m. at the beginning, middle and end of the month respectively. Jupiter is a morning star, rising at 3h. at the beginning and 1h. 33m. at the end of the month. Saturn, in the constellation of Taurus, is a morning star, rising at 22h. 08m. in the middle of the month. Times are given approximately for the latitude of Greenwich. The Autumn Equinox commences on Sept. 23d. 22h. The longer evenings provide opportunities for those who are interested in observational work, and many interesting objects are within the range of a small telescope.

LETTERS TO THE EDITORS

The Editors do not hold themselves responsible for opinions expressed by their correspondents. No notice is taken of anonymous communications.

X-Irradiation of Rabbit Spermatozoa *in vitro*

THE classic experiments of Hertwig on the X-irradiation of frog spermatozoa were recently extended by Rugh[1], who reported that exposure of the spermatozoa to doses from 15 r. to 10,000 r. caused a progressive increase in embryonic mortality; at the higher dosage only 1·6 per cent of the embryos hatched. Further increase of the dosage, however, increased the number of viable embryos; at 50,000 r., 90·5 per cent hatched. These embryos were morphologically uniform and very similar to haploids produced by other means. Rugh concluded that with the higher doses the spermatozoa, which showed normal motility, were able to penetrate the egg, but were unable to effect syngamy, so that the development initiated was parthenogenetic.

Comparable effects with the small doses have been demonstrated in mammals. Doses of 800 r. to mice[2] and 1,000 r. to rats[3] do not diminish the capacity of the existing spermatozoa to effect fertilization, but early mortality among the embryos reduces litter size. The mammalian work on X-rays, with the solitary exception, so far as we know, of some inconclusive experiments by Asdell and Warren[4], has involved the mating of irradiated males, a technique which has several disadvantages. Pincus and Enzmann[5], however, carried out a few experiments on rabbits by inserting into the Fallopian tube spermatozoa which had been exposed *in vitro* to ultra-violet irradiation, and obtained some rather inconclusive evidence of penetration and activation of the eggs without syngamy. We have now carried out, also on rabbits, experiments involving the *in vitro* X-irradiation of semen, followed by artificial insemination. In addition, the induction of superovulation by the injection of gonadotrophin has proved most useful in augmenting the number of eggs used for the study of the segmentation stages.

The females were killed and the tubal or uterine contents examined at various times up to 40 hr., at 9 days, or at 28 days after ovulation. Results with the various dosages were briefly as follows:

50 r. and 100 r. The great majority of the eggs segmented normally and normal implantations at 9 days and normal young at term were also obtained.

250 r. and 500 r. Most of the eggs were segmenting normally at 40 hr., but there was a definite increase in the percentage of those which were retarded or arrested. Normal implantations and apparently normal young at birth were obtained, but in small numbers.

1,000 r. The great majority of the eggs had started to divide but had no more than 10 cells at 40 hr.; less than 3 per cent were apparently normal. No implantations at 9 days or young at term were obtained with this or any higher dose.

2,500 r.–10,000 r. More than 70 per cent of the eggs recovered at 40 hr. had started to segment, but most of these were in the 3- or 4-cell stage. None was segmenting normally.

25,000 r.–50,000 r. Most of the eggs had made a single division only at 40 hr., and were arrested in the 2-cell stage.

100,000 r. Only 8 of the 70 eggs obtained at 40 hr. had divided, and 6 of these were in the 2-cell stage.

Cytological examination was made of many of the eggs fertilized by spermatozoa receiving between 500 r. and 100,000 r. The presence of male nuclei within the cytoplasm of the egg showed that penetration had been effected at all dosages. Spermatozoa were abundant in the perivitelline space and zona pellucida at all dosages up to 50,000 r., but at 100,000 r. very few were seen. Delay in migration and vesiculation of the male nucleus was observed at all doses from 1,000 r. to 10,000 r. and constituted the earliest indication of any organic change caused by irradiation of the spermatozoa. Cytological evidence of damage to the male nucleus appeared only with doses of 10,000 r. and above. At lower doses the nuclei were single and oval or spherical, while with the higher doses the majority were hypertrophied and lobed.

At the stage of fusion of the pronuclei the effects of various dosages may be summarized as follows:

1,000 r. Syngamy was complete with no outward sign of irregularity in the male pronucleus.

10,000 r. Syngamy took place, but the male pronucleus was very irregular and hypertrophied, and the chromosomes remained dispersed.

25,000 r. Syngamy might take place, but in some eggs the occurrence of polyspermy produced derangement in the distribution of the chromosomes. Cytoplasmic division and separation in these cells might be incomplete.

50,000 r. Syngamy occurred, but the formation of the male chromosomes was delayed or suppressed, so that zygotic combination was weakened. Because of the delay in polar migration of the chromosomes the cleavage spindles were irregular.

100,000 r. Extrusion of the polar bodies and migration of the male nucleus was observed, but not syngamy in any of the eggs studied. However, the recovery of 2-celled eggs with diploid chromosomes showed that the absence of syngamy may not be constant. The occurrence in one egg of a single segmentation spindle with haploid chromosomes suggested that spermatozoa exposed to 100,000 r. can sometimes activate the egg without fusion of the pronuclei. No evidence of continued gynogenetic development was, however, obtained.

A full account of the experiments outlined above is being prepared, but it may be noted here that the technique of semen collection, artificial insemination and, where appropriate, induction of superovulation, offers exceptional facilities for the type of work described above, and is being used by us for the study of the effects of chemical and serological as well as of physical agents on the physiological and genetic properties of mammalian sperm.

We are much indebted to Dr. W. V. Mayneord who kindly allowed the irradiations to be carried out in the Department of Physics of the Royal Cancer Hospital.

E. C. AMOROSO.

Histology Department,
Royal Veterinary College,

A. S. PARKES.

National Institute for
Medical Research, N.W.3.
July 26.

[1] Rugh, R., *Proc. Amer. Phil. Soc.*, **81**, 447 (1939).
[2] Snell, G. D., *J. Exp. Zool.*, **65**, 421 (1933).
[3] Henson, M., *J. Exp. Zool.*, **91**, 405 (1942).
[4] Asdell, S. A., and Warren, S. L., *Amer. J. Röntg. and Rad. Therap.*, **25**, 81 (1931).
[5] Pincus, G., and EICHWALD, E. V., *J. Exp. Zool.*, **73**, 195 (1936).

Possible Role of Birds in the Maintenance of Yellow Fever in West Africa

In 1936 it was shown that the blood of monkeys from endemic yellow fever zones in Africa contained immune bodies to the yellow fever virus, as judged by the intraperitoneal mouse protection test: the sera of monkeys outside the endemic zone did not contain immune bodies[1]. These results have since been confirmed by other workers. At the same time, the blood of a sheep from the Gambia was found to possess virucidal bodies against the yellow fever virus. Since then virucidal bodies have been found in the blood of cattle, sheep, dogs, pigs and a camel: the significance of these findings in domestic animals awaits further study, since virucidal bodies were present in the blood of certain domestic animals living outside the commonly accepted yellow fever zone.

Although there is highly suggestive evidence that monkeys may act as an alternative host to man in maintaining yellow fever in Africa, the monkey population is not large, nor is there evidence that monkeys migrate over great distances.

A search has therefore been made for other possible hosts sufficiently numerous to maintain the virus in the absence of man. The sera of a number of animals and birds have been examined from the Gold Coast. Although no epidemic has been reported in this Colony since 1937, single sporadic cases have occurred every year, and immunity surveys in children show that the number of cases reported bears no relation to the number of children giving positive results. Apart from monkey sera giving positive results, a rodent, a Cutting grass *Thryonomys swinderianus*, one of five examined, has been found to possess virucidal bodies to yellow fever virus in its blood. The sera of civet cats, giant rats, bush-tailed porcupines and royal pythons have been negative. More striking results have been obtained with the sera of wild birds. Of forty birds examined four have shown virucidal bodies to the yellow fever virus. The positive birds were a buff-backed heron *Bubulcus ibis ibis*, one of four examined, an African barn-owl *Tyto alba affinis*, one of three examined, and a Senegal kingfisher *Halcyon senegalensis senegalensis*, the only specimen examined. All these birds belong to common and widely distributed species.

Further work, which is at present proceeding, is required to determine the exact significance of the virucidal bodies contained in the sera of these birds. Should further evidence show that the virucidal action is due to the development of immune bodies as a result of previous infection with the yellow fever virus, a clue may have been found to the maintenance of yellow fever in Africa, and possibly also an explanation of the occasional occurrence of sporadic infections in man at considerable intervals in space and time. Much further knowledge is required of the migration of birds within Africa. Thus the Senegal kingfisher is said to be migratory in Darfur, but there is no record of its migration in the western part of its range. The buff-backed heron, however, is known to have an extensive migratory range from the southern edge of the Sahara to the Guinea coast, thus traversing the endemic yellow fever zone from north to south. This bird is closely associated with man and with cattle.

It will be recalled that in America birds appear to play an important part in the maintenance of the eastern and western strains of equine encephalomyelitis virus: the same may be true of the yellow fever virus in Africa.

G. M. FINDLAY.
T. A. COCKBURN.

[1] Findlay, G. M., Stefanopoulo, G. J., Davey, T. H., and Mahaffy, A. F., *Trans. Roy. Soc. Trop. Med. and Hyg.*, **29**, 419 (1936).

Synergistic Effect of Para-aminobenzoic Acid and Sulphapyridine on Penicillin

A SOLUTION of sodium penicillin, containing 1,200 units per c.c., was tested for bacteriostatic activity against *B. subtilis* and found to be active in a dilution of 1 : 100 in a synthetic medium containing 0·1 per cent acid hydrolysed casein[1].

When *p*-aminobenzoic acid was added to the medium in dilutions of 1 : 2,500–1 : 10,000, the inhibitory effect of penicillin on the test organism was increased to a titre of 1 : 6,000. An increase in the activity of penicillin on addition of *p*-aminobenzoic acid was also observed with glucose broth as nutrient medium. A similar, though less marked, effect on penicillin activity was shown by *p*-aminobenzoic acid against *Staphylococcus aureus* (strain 663) in the same synthetic medium containing hydrolysed casein. Using a solution of sodium penicillin with a titre of 1 : 40,000, the effect of *p*-aminobenzoic acid (added in a dilution of 1 : 2,500–1 : 50,000) was to increase the titre to 1 : 75,000–1 : 100,000. *p*-Aminobenzoic acid had no effect on the activity of penicillin solution against *Streptococcus hæmolyticus* (strain 618, group *A*).

p-Aminobenzoic acid did not increase the activity of penicillin in concentrations greater than 1 : 1,000.

The enhancing effect of sulphapyridine on penicillin was more marked than that of *p*-aminobenzoic acid. Sulphapyridine, which, when tested against staphylococcus (strain 663), showed no inhibition of growth in digest broth in dilutions up to 1 : 2,000, was added in dilutions up to 1 : 50,000 to a solution of sodium penicillin, which inhibited at 1 : 30,000; inhibition occurred up to a titre of 1 : 70,000. When *p*-aminobenzoic acid was added in a dilution of 1 : 20,000 to the penicillin solution containing sulphapyridine (1 : 5,000), inhibition occurred at 1 : 40,000.

The experiment was repeated using 1 per cent glucose broth, and the activity of penicillin against staphylococcus (strain 663) and streptococcus (strain 618) was again shown to be enhanced in presence of sulphapyridine. A solution of sodium penicillin inhibited staphylococcus at a titre of 1 : 20,000; in presence of sulphapyridine (in dilutions up to 1 : 50,000) the solution inhibited at a titre of 1 : 50,000. Similarly, a solution of sodium penicillin inhibited streptococcus (strain 618) at dilutions up to 1 : 50,000; in presence of sulphapyridine (in dilutions from 1 : 5,000 to 1 : 50,000), the same penicillin solution inhibited streptococcus at a dilution of 1 : 100,000. Sulphapyridine itself showed inhibition only at a concentration of 1 : 2,500. A dilution of 1 : 5,000 had no effect on the growth of steptococcus.

Thus, the inhibitory effect of penicillin in presence of small quantities of sulphapyridine, which, *per se*, are insufficient to show any effect on staphylococcus or streptococcus, is at least doubled.

Having shown *in vitro* the synergistic effect of penicillin and sulphapyridine, their action was investigated *in vivo* in mice, and some preliminary

results confirming the *in vitro* experiments are recorded.

Groups of six mice were treated with (*a*) small doses of sulphapyridine insufficient to give any protection either against streptococcus or staphylococcus, (*b*) small doses of penicillin, (*c*) both substances simultaneously in these doses.

Sulphapyridine was injected in 0·5 per cent suspension and given in doses of 0·1 c.c. twice daily intraperitoneally. A solution of sodium penicillin (about 90 units per c.c.) was injected in 0·2 c.c. doses, also twice daily. Treatment was continued for four days and the results were recorded at the end of seven days from the commencement of treatment. Each group of mice was infected, at the beginning of treatment, with 1×10^6 streptococci (strain 618) or with 500×10^6 staphylococci (strain 663). The table shown below gives the number of survivals.

	Streptococcus haemolyticus							*Staphylococcus aureus*						
Days . .	1	2	3	4	5	6	7	1	2	3	4	5	6	7
Sulphapyridine	6	2	1	1	1	1	1	0	0	0	0	0	0	0
Sodium penicillin . .	6	5	2	2	1	1	1	4	4	2	2	1	1	1
Sulphapyridine + sodium pen. . .	6	6	6	6	6	4	4	5	5	4	4	4	4	1
Control Strept.	0	0	0	0	0	0	0	1	0	0	0	0	0	0
					↑ Treatment stopped							↑ Treatment stopped		

The results show the increased protective effect of both substances given together. This might give an even greater survival rate if treatment were extended for more than four days.

The increased activity of both substances when applied simultaneously may be due either to the synergistic action of the two substances on the micro-organism, or less probably to a chemical reaction between the sulphonamide and the penicillin. These possibilities are being investigated.

We thank the directors of the Therapeutic Research Corporation of Great Britain, Ltd., for permission to publish this communication.

It should be pointed out that penicillin is not yet available for general use.

J. UNGAR.

Glaxo Laboratories, Ltd.,
Greenford, Middlesex.
June 17.

[1] Ungar, J., NATURE, 152, 22 (1943).

The Development of the Angoumois Grain Moth (*Sitotroga cerealella* Oliv.)

THE Angoumois grain moth has been known as an important pest of stored and field grain since the time of Réaumur[1], and it has possibilities as a laboratory insect. A search through the literature[1–9] revealed, however, that its life-history has apparently never been worked out in detail. A knowledge of the number and duration of the larval instars at 30° C. and 70 per cent relative humidity was required for a quantitative study of populations of this species, and some observations were consequently made under these experimental conditions.

One freshly laid egg was introduced into each of a hundred small vials. The latter were then examined daily until the eggs hatched, when one gram of fine wheat flour was added to each vial. The flour had been obtained from a local miller and sieved through No. 52 (extra quadruple) silk bolting cloth. The larvæ of Sitotroga will develop quite successfully in this medium, although mortality is high. The contents of each vial were gently sieved daily, and moults deduced from the discovery of cast skins and checked by measuring the width of the head capsule. Four larval instars were found, followed by a pupa, from which the adult eventually emerged. Mature larvæ were found in hollows in the flour, lined by a delicate cocoon, against the glass side of the vial. They became quiescent for about two days before metamorphosis. The average duration and body measurements of each instar are shown in the accompanying table. The average ratio of increase in each instar, used in the calculations from Dyar's rule, was 1·7.

Instar	Duration in days		Width of head capsule (mm.)	Width of head capsule, calculated from Dyar's rule	Approx. width of thorax (mm.)	Approx. total length (mm.)
	Mean	Range				
Egg	3	2– 7				
First larval	6	4–10	0·15–0·19		0·22	1·2
Second larval	6	4–10	0·28–0·32	0·29	0·38	1·6
Third larval	5	2– 9	0·47–0·53	0·51	0·80	4·3
Fourth larval	7	6–12	0·76–0·84	0·85	1·65	7·0
Pupa	5	4– 7				
Total	32	30–40				

The body measurements are each the averages of observations of five individual larvæ, and the developmental periods the averages of observations of at least twenty-one individuals. Only twenty-two adults emerged from the hundred original eggs, twenty having been killed to make body measurements and fifty-eight dying. Twenty-one of these adults emerged within ten days of each other, while one did not emerge until the seventy-second day after oviposition. Such prolonged development has been observed by other authors[2,3], but under uncontrolled conditions of temperature and humidity. This abnormal individual was disregarded in calculating the average duration of the different instars. All its larval instars were longer than the average, while the pupal period was of average duration. The moths usually mated almost immediately upon emergence, and the pre-oviposition period was approximately one day. The average generation time was thus thirty-three days.

This is a suitable opportunity to correct an omission from a previous paper[4]. In line 11 from the bottom of p. 330, for "insects per grain" read "insects per grain, at which densities the average longevity was 6·6 and 4·4 days, respectively".

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Frictional Properties of Metallic Films

DR. HUGHES's recent remarks[1] would appear to be based on the three wrong premises that, first, films of soft metals wiped on to steel surfaces on which a loaded steel ball was made to rotate[2] never decreased the electromotive force; secondly, the interpretation of earlier observations[3] on the frictional behaviour of such films on a copper surface made on the Bowden-Leben apparatus had been criticized; and thirdly, frictional decreases due to these films should be taken as an indication that changes in the ploughing contribution to the frictional force were immaterial when such decreases were observed "during the *first* slide" with the Bowden-Leben apparatus.

Dr. Hughes, it seems, has overlooked the difference of the results obtained with the revolving sphere and metallic films wiped out once under 'dry' conditions, and the other time in the presence of an oil. It has been stated[2] that "Even in those cases in which the films reduce the electromotive force under 'dry' conditions, they do not appear to function as a lubricant for a brief interval until normal lubricating conditions are re-established".

The results listed in Table 2 (ref. 3) allow of no misrepresentation, and it is the conclusion drawn by Dr. Hughes and his colleagues[3] from a comparison of Tables 1 and 2 (ref. 3) that "in a heterogeneous bearing metal the soft phase wipes out over the hard phase during sliding" that is regarded as conjectural.

There is a difference between wearing in a groove on the surface of a block of compact metal and of ploughing a track on a film of wiped-out softer metal of sufficient thickness to carry the main portion of the normal load. To choose a homely analogy, it is qualitatively the same difference as between scribing a line with the end of a walking-stick once on a hard rolled gravel path and then on the same path covered with a reasonable amount of loose gravel. One is bound to experience a frictional decrease "during the *first* slide" in the latter case. Just on account of the ploughing action would one expect a smaller friction "during the *first* slide" in the presence of a wiped-out metallic film, the friction remaining little changed by successive strokes in the same track, *until* the wiped-out films would be penetrated, when the friction would be expected to *rise* to values approaching that of steel on the substrate—copper, for example—and on further continued successive strokes in the same track a renewed decrease of the friction would be expected with the wearing in of *one* groove in the substrate.

There seems to be general agreement[1,2,4,5] that ploughing is the essential feature of the Bowden-Leben apparatus when the hemisphere is the harder of the two friction elements, since Dr. Hughes's claim has remained unsubstantiated "that the grooving contribution to the friction, which must evidently be present to a certain extent, is small compared with the contribution of adhesive forces".

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IN the light of Dr. Schnurmann's above remarks, it appears that he is still unconvinced by our original interpretation of frictional data[1] on the Bowden-Leben apparatus, when a small steel spherical surface was slid over copper covered with thin films of soft metals (lead, cadmium and tin), and also by later additional experimental data[2]. It was originally suggested that the reduction in the coefficient of friction, μ, from 0·5 to 0·7 for steel on the bulk metals lead and copper, say, to 0·03–0·08 for films of lead, 10^{-3}–10^{-4} cm. in depth, was due to the combination of a thin soft film on a hard substrate maintaining a small area of contact across which "the adhesion and deformation resistance" was very low. Dr. Schnurmann considers the effect is solely due to a reduction in the "ploughing action".

It would perhaps clarify the argument if an attempt is made to define these terms. The frictional force may be considered to be made up of two contributions[3,4]:

(i) The *adhesion forces* across the real area of contact. This is the shear strength of the metal junctions in Dr. Bowden's recent publications[4,5,7].

(ii) The *deformation forces* required to plough out a groove. This is the ploughing term, the magnitude of which for indium films on steel has been investigated by Bowden, Moore and Tabor[5]. This contribution to the friction may be defined as the force required to deform the metal in the path of the slider, and would contribute to the total friction even if the adhesional forces across the area of contact became zero. The greater drag of a ski on soft snow above that on hard crusted snow is perhaps a simple illustration of a case where the ploughing term is relatively large.

If we consider our original experiments, when a steel ball 2 mm. in diameter was slid over copper and copper covered with a thin film of lead (which recent measurements[6] have shown need only be about 5×10^{-6} cm. in depth for the minimum friction), then since the track width is of the order of 0·02 cm., a simple calculation will show that the area of contact on the bare and lead-covered copper will be practically the same in both cases, since the thin film thickness is only a small fraction of the depth of the sliding groove. This must indicate that the ploughing term is approximately the same in each case. However, since the friction experiments show a tenfold drop in μ to accompany the deposition of this lead film, it must be concluded that it is the lower adhesion (or shear strength) between the lead and the steel slider compared with that between copper and steel which is responsible for the drop in μ.

This argument has been put on a quantitative basis in Dr. Bowden's recent publications[4,5,7]. By comparing different forms of slider on soft indium films deposited on hard steel, he has been able to estimate the ploughing and adhesion contributions to the friction for comparatively wide track widths up to 0·2 cm. Thus Fig. 9 of ref. 5 shows that the adhesion term is much greater at low track widths, that is, for the thinnest films such as those used by us. The proportionality between the square of the track width (that is, the area of contact) and the frictional force has been used to show the dependence of the friction on the shear strength across the area of contact, that is, on the adhesion term. It has not been used to support the ploughing hypothesis, as Dr. Schnurmann implies above.

Dr. Schnurmann's analogy of a walking-stick sliding on a gravelled path is perhaps rather ill-chosen on account of complications due to grain

rolling; we prefer our original analogy of the lubricating action of lead on a copper substrate being similar to that of a thin film of mud on a smooth road, when the area of contact between a pedestrian or motor vehicle and the road is little different from that on the dry road, while the fall in kinetic friction is a matter of common experience.

Incidentally, Dr. Schnurmann's expectation of a rise in μ followed by a further decrease after breaking through the lead film on copper is at variance with facts. Experiments show the friction to increase from $\mu = 0{\cdot}05$–$0{\cdot}08$ to $\mu =$ greater than $0{\cdot}3$ as the lead film is worn through, until as continued sliding causes copper to pick up, μ may reach a value above unity, and never decreases with further sliding.

If Dr. Schnurmann's results[8] have been incorrectly represented in a previous communication[3], one of us (T. P. H) must plead a misinterpretation of the experimental observations, which on account of their brevity were perhaps somewhat ambiguous. It is, however, difficult to see how the thermo-electric E.M.F. between pairs of sliding surfaces covered with various metal films (each metal having a different thermo-electric E.M.F.) can be considered as a certain measure of the friction μ. No doubt this point will be cleared up when Dr. Schnurmann's detailed results are published at a later date.

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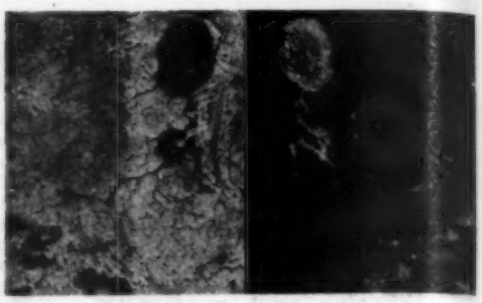
Recording Low Relief in Metallic Surfaces by Cellulose Acetate Moulds

A THIN transparent sheet of cellulose acetate, bearing on one face an accurate mould of the low relief produced in a relatively flat metal surface by abrasion, etching or impact, constitutes a useful permanent record of this relief for comparative purposes. It is superior to a photograph in being three-dimensional, but serves as a negative for photographic copies, if necessary.

Such a mould reproduces both megascopic and microscopic details with exceptional clarity. In particular, it facilitates (*a*) studies of crystalline structures revealed by polished and etched serial sections of ingots, and (*b*) investigation of the changes produced in crystalline structures of polished and etched metallic test specimens by different types of mechanical working.

Recently the method has been utilized for examining polished and etched plane sections of metallic meteorites, iron ores, steels, and brasses. Previously I employed this procedure for investigating the finer structures of rock and mineral surfaces[1]. It is allied to the collodion-mould technique of A. G. Nathorst[2], and the 'peel' technique of J. Walton[3] for studying fossil plants.



POSITIVE PRINT (RIGHT) AND ITS PAPER NEGATIVE DERIVED FROM A CELLULOSE ACETATE MOULD (LEFT) OF THE POLISHED AND ETCHED SURFACE OF A METALLIC METEORITE FROM CAÑON DIABLO, ARIZONA, U.S.A. (HUNTERIAN MUSEUM COLLECT., UNIVERSITY OF GLASGOW).
The mould was 0·08 mm. in thickness and the paper negative made on "Kodaline Slow Paper"; exposure 15 sec. at 30 in. from 100-watt lamp.

A mould is produced from a colourless or dyed dispersion of cellulose acetate in one part by volume of tetrachloroethane and two parts by volume of "Cerric Thinner T. 10" (Cellon, Ltd., Kingston-on-Thames) by (*a*) wetting the cleansed metal surface with some of the dispersing medium; (*b*) levelling and then flowing over this surface a layer of the dispersion, 1–1·5 mm. in depth; (*c*) removing the resulting cellulose acetate film from the surface when volatile parts of the dispersing medium have evaporated; that is, in eight to twelve hours after flowing the layer. Premature stripping of this film from the metal (for example, in four hours) may result in distortion of relief arising from slight shrinkage of the mould parallel to the plane of the metal surface.

This simple process yields strong transparent moulds of refractive index $N_D^{20^\circ}$ 1·49, which are usually between 0·1 mm. and 0·2 mm. in thickness, as much as 1,200 sq. cm. or more in area, light in weight, very flexible, and insoluble in water, cold concentrated hydrofluoric acid, hot Canada balsam and xylol balsam. If such a film is to be kept for reference, its flexibility can be preserved, and ultimate shrinkage reduced, by adding to the dispersion a cellulose acetate plasticizer, such as triphenylphosphate or dimethyl-phthalate, in proportion to the percentage of cellulose acetate present.

The metallic surface is not damaged in any way by the moulding process, but any dust or parts of oxide or other detachable films are removed, and retained by the mould.

For comparison, moulds can be superposed and examined over an illuminated tracing desk. Alternatively, critical parts of them can be cut out and mounted in 'cooked' Canada balsam, for examination under the microscope at any convenient magnification up to about 500 diameters, or by optical projection.

The utility of the method is now being tested in connexion with slags, glasses, ceramic products and refractories.

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A Neglected Constituent of Proteins, α-Amino-*n*-butyric Acid

ALTHOUGH its presence in proteins has been reported several times[1-6], α-amino-*n*-butyric acid has not been generally accepted as a naturally occurring amino-acid, probably because no readily reproducible method for its isolation has yet been described. However, the isolation of its hydroxy homologue threonine[7] suggests that α-amino-*n*-butyric acid may occur widely, since whenever a known hydroxy amino-acid occurs in any given protein, the corresponding simple amino-acid is usually present in even larger amounts. Accordingly, it seems probable that an appropriate fractionation of the alanine and valine fractions of a protein rich in threonine would yield appreciable amounts of α-amino-*n*-butyric acid. Hydrolysis should preferably be enzymatic, since prolonged heating with mineral acids has been found to liberate some nitrogen from the synthetic *dl*-α-amino-*n*-butyric acid[8], while *l*-glutamic acid heated with alkalis decomposes to acrylic acid and α-amino-*n*-butyric acid[9], so that misleading results would be obtained by alkaline hydrolysis of proteins. Preliminary work along these lines has been interrupted by the War.

Meanwhile, synthetic *dl*-α-amino-*n*-butyric acid has been increasingly used in comparative studies on amino-acids. No appropriate common name for the amino-acid has yet been proposed. Since the full chemical name is somewhat cumbersome, the term *quadrine* is suggested as a common name. This is intended to call to mind the fact that the acid contains four carbon atoms. α-Amino*iso*butyric acid could be designated as *isoquadrine*.

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Cleavability of Keratins Treated with Hot β-Naphthol by Proteinases

WHEN sheep wool is heated with β-naphthol at a temperature of 140–150°, it dissolves; and when after cooling the β-naphthol is removed with ether, an amorphous yellowish substance is obtained in a yield of about 80 per cent of the weight of the wool. This product is readily hydrolysed by a glycerol extract of pancreatin, purified pancreatic proteinase (free of peptidases and protaminase), pepsin, and taka-diastase. Yeast polypeptidase does not affect the product but causes additional hydrolysis after a prior treatment of the product with pepsin.

In a similar manner, chicken feathers were dissolved in hot β-naphthol. The product that remained after removal of the β-naphthol is almost completely soluble in hot 70 per cent alcohol and is precipitated from this solution by an excess of absolute alcohol. The product so obtained is readily cleaved by a glycerol extract of pancreatin, by purified pancreatic proteinase and by pepsin. Like the product obtained from wool, it is resistant to yeast polypeptidase but is cleavable by the latter after a previous exposure to the action of either pepsin or purified pancreatic proteinase.

The products obtained from the two keratins thus behave toward proteolytic enzymes like the 'acropeptides' earlier obtained in a similar manner from other proteins by A. Fodor and collaborators[1].

The experimental part of this investigation was carried out in collaboration with Miss S. Cukerman and Mrs. I. Fodor-Salomonowicz. A detailed report will be published elsewhere.

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[1] cf. "Annual Review of Biochemistry", **9**, 47 (1940).

Vacuum Filtration of Sewage Sludge

VACUUM filtration of sewage sludge, particularly surplus activated sludge from the activated sludge process of sewage treatment, is a well-established process, particularly in the United States, for dewatering sludge prior to drying and incineration or preparation for use as fertilizer or disposal by other means. It has been found from long experience that for successful filtration the sludge must be coagulated beforehand. The coagulant generally used is a ferric salt, usually ferric chloride, and many attempts have been made to reduce the amount of coagulant required with the view of making the process more economical.

Experiments are being conducted on these lines at Manchester Sewage Works, and the finding of other workers[1] has been confirmed that the fresher the sludge the less ferric chloride is required for coagulation. It has also been found that the better the 'condition' of activated sludge from a sewage purification point of view the less ferric chloride is required for coagulation. When activated sludge is kept for periods up to two days, it becomes more alkaline, and this increases the ferric chloride demand. When activated sludge, or a mixture of activated sludge and sedimentation tank sludge, is stored, reducing substances are produced, and these by reducing part of the ferric salt to ferrous salt increase the ferric chloride demand. Aeration of stale activated sludge, or a mixture of activated sludge and sedimentation tank sludge, for a short period (one to two hours) reduces the alkalinity and appreciably reduces the amount of ferric chloride required for coagulation. This may be due in part to oxidation of the reducing substances referred to above. Aeration is beneficial even with the freshest sludge obtainable from the activated sludge plant.

We believe that aeration of sewage sludge, as a preliminary to coagulation and vacuum filtration, has never before been reported. It is hoped shortly to publish the results of the experiments.

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INFECTION OF LABORATORY ANIMALS WITH JOHNE'S DISEASE AND LEPROSY

By JOHN FRANCIS

JOHNE'S disease chiefly affects the bovine. It is a chronic bacterial enteritis caused by *Mycobacterium johnei* which produces proliferative changes in, and a rather characteristic thickening of, the intestinal mucosa. The bacteria are usually confined to the intestine and adjacent lymphatic glands. Animals acquire the infection when very young, but the incubation period is protracted, symptoms usually not occurring until two to three years of age or even later; calving or other stresses may cause the onset of symptoms. Clinical recovery may occur, but usually the diarrhœa and emaciation are progressive, eventually resulting in death.

Several workers have remarked on the similarities between the histopathology of Johne's disease and leprosy, and in both the bacteria are largely intracellular. The respective organisms resemble each other in that neither will grow on the usual culture media, and until recently it was generally accepted that laboratory animals had not been infected with *Myco. lepræ*, or with *Myco. johnei*. There was, however, considerable evidence that even naturally susceptible animals became resistant to infection with *Myco. johnei* at an early age, but there was no evidence that young laboratory animals had ever been used in experimental work[1]. I therefore attempted to produce infection by inoculating very young laboratory animals.

Four mice, fourteen days old, were each given intraperitoneally 2·5 mgm. moist bacilli from a culture of *Myco. johnei*. (The organisms were grown on an egg medium containing an extract of *Myco. phlei*. The use of this extract was first described by Twort and Ingram in 1913, see ref. 2.) The animals were re-inoculated with the same dose seven days later. Post-mortem examinations were carried out 9–34 weeks after inoculation, and showed that a progressive infection, commencing in the liver and spleen and eventually involving almost the whole of the intestinal tract, had developed. Seven mice, thirteen days old, received liver tissue from one of these infected mice. The bacilli multiplied in the second group of mice more rapidly than in those originally inoculated. The results of a second mouse passage were largely vitiated due to the early death of the inoculated mice. Another group of thirteen-day-old mice was inoculated with a total dose of 1·4 or 1·25 mgm. of culture from the spleen of an infected mouse. Three of these mice rapidly became heavily infected, but two, killed later, were negative and a third was only lightly infected. Slight infection was also produced in young hamsters, but on the basis of their weights the doses they received were approximately three times smaller than those given to mice. The result was less consistent, but bacilli were carried through one direct animal passage. The cellular reactions in infected mice and hamsters were similar to those in naturally occurring Johne's disease.

Young rabbits and guinea pigs appeared to be less susceptible to Johne's disease than mice or hamsters.

The numerous attempts to infect laboratory animals with *Myco. lepræ* have been reviewed by McKinley[2] and only a few of the more recent need be considered here. Adler[3] splenectomized four 'young' hamsters (*Cricetus auratus*). A fragment of lepra nodule was placed subcutaneously in the operation wound of three of them and in the fourth a fragment was placed in the thigh muscle; material was also inoculated intraperitoneally. One died and no post-mortem examination was performed. The other animals were killed forty days after inoculation. It was claimed that in the remaining two hamsters inoculated subcutaneously local multiplication had occurred and that bacilli were found in the liver. Bacilli were found in the adjacent lymphatic gland of the hamster inoculated in the thigh muscle.

Burnet[4] inoculated six three-month-old hamsters subcutaneously with fragments of leprous tissue. Three of them were splenectomized and three normal. They were inoculated again 15 weeks later and killed 150–220 days after the first inoculation. Only one of the hamsters, which had not been splenectomized, was infected. It had masses of bacilli at the inoculation site and large numbers in the spleen, liver, kidneys, some lymphatic glands and in a testicular abscess. Inoculation of tissue into guinea pigs showed that this animal was not affected with mammalian tuberculosis.

Burnet and Jadford[5] fed large amounts of leprous skin and spleen to a hamster over a period of twelve days. The hamster died 300 days later after having shown symptoms of malaise. Numerous acid-fast bacilli were found in the spleen and lung, and it was concluded that an active infection had been produced.

De Souza-Araugo[6] inoculated five hamsters (*Cricetus cricetus*) subcutaneously and intraperitoneally with human leprous material; they were given a second subcutaneous inoculation 75 days later. Bacillæmia was demonstrated in three hamsters 163 and 192 days after the first inoculation. Ota and Nitto[7] failed to infect hamster (*Cricetus cricetus*). De Souza-Araugo[8] and Dhamendra and Lowe[9] criticized the validity of Adler's claim to have produced a true infection in hamsters and the latter workers attempted to infect twenty-six adult hamsters, half of which had been splenectomized. Leprous material was inoculated subcutaneously and intraperitoneally but a progressive infection was not produced. Burnet[9] failed to infect hamsters following intra-nasal inoculation.

De Souza-Araugo[10] reported extensive attempts to infect laboratory animals. He claimed[6] that of fifty-nine adult mice, inoculated intraperitoneally with leprous material, 92 per cent developed "systemic infection" (ref. 3). An examination of the results indicates, so far as one can judge, that six or perhaps nine animals developed a true infection. Attempts to infect rats and guinea pigs were less successful.

Jordan[11] inoculated material from typical cases of skin leprosy intradermally into 3–4 month-old white rats. In some of the animals a superficial tumour was produced after 1½–1¾ years. Tissue from these tumours was used for further inoculations. In all, seven passages were carried out in rats and the incubation period was reduced to three–four months. After the first passage, lesions developed in the lung, liver, spleen and kidney; bacilli were not found in the nose. Guinea pigs inoculated with infected material did not become tuberculous. The possibility of the infection being rat leprosy was considered and dismissed: twenty uninoculated rats of the same strain as those used in the experiments died of intercurrent infection, and no acid-fast bacilli were found in their organs. Photomicrographs of lesions in the liver and spleen of infected rats show that the tissue

reaction was of the same type as that seen in mice infected with *Myco. johnei*. These are by far the most convincing results so far reported, but they have not been confirmed by other workers and no subsequent paper by Jordan can be traced. Fite[12] inoculated 154 rats, some of which were young, subcutaneously with human leprous material. Six developed a local leprous lesion after an incubation period of 17½–18 months, and in two of them the possibility of a spontaneous rat leprosy infection was as nearly as possible excluded. Material from two rats was inoculated subcutaneously into twelve other rats, ten of which became infected; a second transfer resulted in uniform takes in twelve rats. Unfortunately, the possibility of these rats becoming infected with rat leprosy was not completely excluded. Tisseuil[13] failed to produce progressive infection in rats inoculated subcutaneously with leprous material suspended in various excipients.

Nanoka[14] failed to produce a progressive infection in young chickens inoculated in the breast muscles with leprous material, but Ota and Nitto[7] claim to have carried out six consecutive passages in fowls by mixing the leprous emulsion with Kieselgühr, trypan blue and potassium iodide.

Taken together, the attempts to infect laboratory animals with leprosy are disappointing. Summarizing his results, Burnet[15] states that, of thirty-five hamsters and sixty-three other animals of various species, only two hamsters were infected (these animals have already been described). He concludes that the exact susceptibility of the hamster to *Myco. lepræ* is not yet known, but that it is certainly variable and probably not much greater than that of mice and rats. One feels that the results obtained by De Souza-Araugo[10] in mice and Jordan[11] in rats were quite as promising as those so far obtained with hamsters.

The difficulty of infecting laboratory animals with leprosy is not surprising, because, of some hundred and forty-five experimental attempts to infect man, no fully accepted positive results have been obtained[1]. But leprosy also resembles Johne's disease in that there is a very long incubation period, and children are much more susceptible than adults. The Philippine Islands Leprosy Commission concluded that children are especially susceptible, and that for all practical purposes adults may be considered as immune (Dunham[16]). Rogers and Muir[17] also say that in the control of leprosy "the protection of highly susceptible children is the key to the solution of the problem". They point out that the negative results of the experimental attempts to infect man are of little significance because they were all carried out on adults who had passed the most susceptible age period. They emphasize that in each of four cases of reported infection through vaccination, the patients were children or adolescents of the most highly susceptible age period. They also record two cases in which a child became infected as a result of thrusting a thorn or a needle into himself after it had been removed from the flesh of a leper. "These last two cases have a special significance owing to their having occurred in children of a highly susceptible age, while two such cases can hardly have both been coincidences."

In all the attempts to infect laboratory animals with leprosy, only a few workers have used 'young' animals. There have, however, been no systematic attempts to ascertain the age susceptibility of any species. The similarities between *Myco. johnei* and *Myco. lepræ* and the diseases they produce have been described. In view of the successful results which have been obtained by inoculating very young mice with *Myco. johnei*, it is felt that an attempt to produce *Myco. lepræ* infection by inoculating very young laboratory animals would be fully justified.

Summarizing, neither *Myco. johnei* nor *Myco. lepræ* will grow on ordinary culture media; they also resemble each other in that they are largely intra-cellular parasites and produce the same type of tissue reaction. Adults, even of the naturally susceptible species, are at least partially resistant to Johne's disease and leprosy, and in both there is a long incubation period. Attempts to produce either of these diseases in laboratory animals have not generally been successful. Mice have now been infected with *Myco. johnei* by inoculating them when very young, and it is concluded that an attempt to produce *Myco. lepræ* infection by inoculating very young laboratory animals might meet with success.

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[2] McKinley, E. B., *Int. J. Leprosy*, **7**, 1 and 217 (1939).
[3] Adler, *Lancet*, ii, 714 (1937).
[4] Burnet, E., *Arch. Inst. Pasteur, Tunis*, **27**, 327 (1938).
[5] Burnet, E., and Jadford, H., *Bull. Acad. Méd. Paris*, **122**, 383 (1939).
[6] De Souza-Araugo, H. C., *Int. J. Leprosy*, **9**, 63 (1941).
[7] Ota, M., and Nitto, S., *Int. J. Leprosy*, **9**, 299 (1941).
[8] Dhamendra and Lowe, J., *Indian J. Med. Res.*, **28**, 61 (1940).
[9] Burnet, E., *Arch. Inst. Pasteur, Tunis*, **29**, 174 (1940).
[10] De Souza-Araugo, H. C., *Trans. Roy. Soc. Trop. Med. Hyg.*, **24**, 577 (1931).
[11] Jordan, P., *Arch. Schiffs-. u. Tropenhyg.*, **40**, 92 (1936).
[12] Fite, G. L., *Publ. Health Rep., Wash.*, **56**, 1919 (1941).
[13] Tisseuil, J., *Bull. Soc. Path. Exot.*, **32**, 542 (1939).
[14] Nanoka, N., *Kitasato Arch.*, **17**, 175 (1940).
[15] Burnet, E., *Arch. Inst. Pasteur, Tunis*, **29**, 155 (1940).
[16] Dunham, G. C., *Amer. J. Publ. Health*, **26**, 27 (1936).
[17] Rogers, L., and Muir, E., "Leprosy" (Bristol: John Wright and Sons, 2nd Edit., 1940).

EVOLUTION OF THE PLANETARY SYSTEM

WITHIN the last twelve months, a considerable amount of interest has been shown in theories of the origin of the planets and satellites. Two new theories have been advanced, notices of which appeared in NATURE (**150**, 215, and **151**, 200), and now another theory by Lieut.-Colonel K. E. Edgeworth has just been published (*J. Brit. Astro. Assoc.*, **53**, 6; July 1943).

The author starts with a vast cloud of scattered material—gas, dust and small particles with an average diameter of 0·5 cm.—and assumes that this cloud was rotating with angular velocity increasing towards the centre. Owing to inelastic collisions, random velocities were reduced and the rotating cloud became a thin disk in which there occurred an increase in local densities, instability finally taking place. Local condensations, each possessing its own rotation, were formed, but these rotations were slowed down through viscosity, and there was an increase in the central density of each condensation. In many cases these condensations broke up into smaller ones but some coalesced into a number of larger ones by their attraction, and from these larger agglomerations the planets and satellites were formed. The mechanism by which the planets were formed was not necessarily the same as that which was responsible for the formation of the sun, and different suggestions are made regarding this last point, but the author hopes that further investigation will decide which view will be most likely.

The satellites were formed in a similar manner to the planets, the scattered material surrounding the planets forming local condensations which coalesced to form satellites. In the case of the moon and other large satellites, like Titan and Triton, it is suggested that this mechanism was not in operation, and it is thought that the moon was formed of the same primitive material as the earth. The asteroids were formed from condensations which failed to coalesce into larger agglomerations, the perturbations of Jupiter being responsible for this effect. The comets were formed in a cloud of scattered material which may still extend far beyond the orbit of Pluto. Owing to the diffuse condition of the material the condensations did not coalesce, merely condensing on self. While the asteroids developed sufficient cohesion to form solid bodies, the comets remained a mere collection of loose particles. Probably the great difference of temperature was responsible for this divergence of development.

The theory is stated in a very tentative form and a large amount of detail still remains to be developed. No doubt the author will clarify a number of points which appear to present certain weaknesses in the theory.

THE MUMBWA CAVES, NORTHERN RHODESIA

THE Mumbwa caves have long been known. Yet, in spite of the fact that in South Africa prehistoric cave sites are few and far between, only now has efficient and systematic investigation been undertaken. F. Macrae, many years ago, was the first to make some trial excavations, the resulting finds from which are in the Cambridge Museum of Archæology and Ethnology, but no complete investigation was attempted. Macrae unearthed a really fine Acheulean type of *coup de poing* in the basal level, and this was covered by other layers containing poor Middle Stone Age industries. A characteristic feature was the number of spherical stone balls found. Now at last a detailed account of the whole section actually present has been prepared ("Further Excavations (1939) at the Mumbwa Caves, Northern Rhodesia", by J. Desmond Clark, *Trans. Roy. Soc. of S. Africa*, 29, Pt. III).

At once it must be remarked that the so-called furnace with iron slag said to have been found in the middle levels by a previous investigator (not Macrae) has been shown to have probably been a grave, while the 'slag' has been proved to be merely a brown cave deposit of sand and silt cemented by calcium phosphate and carbonate. Clark found no more lower palæolithic artefacts, and it would seem that the inhabitants of the Mumbwa sites in those times cannot have been numerous. Macrae was evidently lucky. Above the lowest level comes an industry called by Clark the Rhodesian Stillbay, and this in turn is overlain by a Northern Rhodesian Wilton, the whole being capped by a layer containing iron objects. The finds are well illustrated and carefully analysed, and comparisons are made with other discoveries in the region and at the classic sites.

A section on the geology of the caves has been supplied by F. E. Zeuner, and the author has had the advantage of collaborating with I. F. Schofield when describing the Iron Age culture from the uppermost layer. The article as a whole is a very useful piece of work.

M. C. BURKITT.

FORTHCOMING EVENTS

Saturday, September 4

BRITISH PSYCHOLOGICAL SOCIETY (at Tavistock House, Tavistock Square, London, W.C.1), at 2 p.m.—"On the Problem of Representative Sampling of the Population".

APPOINTMENTS VACANT

APPLICATIONS are invited for the following appointments on or before the dates mentioned:

TEACHER OF MINING SUBJECTS—The Education Officer, County Hall, Wakefield (September 1).

EDUCATIONAL PSYCHOLOGIST AT THE CHILD GUIDANCE CLINIC—The Chief Education Officer, Education Offices, Northern Secondary School, Mayfield Road, Portsmouth (September 4).

ADVISORY MYCOLOGIST—The Secretary, North of Scotland College of Agriculture, 41½ Union Street, Aberdeen (September 4).

LECTURER IN ELECTRICAL ENGINEERING of Degree standard for National Certificate Course, at the South-East London Technical Institute, Lewisham Way, S.E.4—The Education Officer (T. 1), County Hall, Westminster Bridge, London, S.E.1 (September 4).

TEACHER FOR THE CORRECTION OF SPEECH DEFECTS—The Director of Education, Education Offices, Town Hall, Hanley, Stoke-on-Trent (September 11).

HEAD OF THE DEPARTMENT OF MATHEMATICS—The Principal, Heriot Watt College, Edinburgh (September 20).

HEADS OF THE DEPARTMENTS OF PHYSIOLOGY, MEDICINE, AND SURGERY—The Bursar, Royal Veterinary College and Hospital, at The University, Reading (October 1).

CHAIR OF NAVAL ARCHITECTURE—The Acting Secretary of University Court, The University, Glasgow (October 4).

DIRECTOR OF ROBERT GORDON'S TECHNICAL COLLEGE—The Secretary, Robert Gordon's Technical College, Aberdeen (October 16).

LECTURER IN ELECTRICAL ENGINEERING for part-time students taking London University Examinations to Final B.Sc. and Higher National Certificate standards—The Principal and Organizer of Further Education in Rugby, College of Technology and Arts, Eastlands, Rugby.

TEACHERS FOR EVENING CLASSES IN CHEMISTRY up to Intermediate Science standard—The Director of Education, The Polytechnic, 309 Regent Street, London, W.1.

ASSISTANT PORT HEALTH OFFICER for the Basrah Port Directorate, Government of Iraq—The Secretary, Overseas Manpower Committee (Ref. 1034), Ministry of Labour and National Service, Alexandra House, Kingsway, London, W.C.2.

DEPUTY CHIEF CHEMIST—The General Manager, Liverpool Gas Company, Bold Street, Liverpool 1 (endorsed 'Chemist').

REPORTS and other PUBLICATIONS

(not included in the monthly Books Supplement)

Great Britain and Ireland

Medical Research Council. Special Report Series, No. 244: Chronic Pulmonary Disease in South Wales Coalminers, 2: Environmental Studies. A.: Report by the Committee on Industrial Pulmonary Disease; B.-G.: Reports on Physical, Chemical and Petrological Studies, by Dr. T. Bedford and Dr. C. G. Warner; Prof. H. V. A. Briscoe, Dr. P. F. Holt, Dr. N. Spoor and others; Dr. G. Nagelschmidt; Dr. A. Brammall and Major J. G. C. Leech; D. Hicks and Dr. G. Nagelschmidt; J. Ivon Graham and Dr. D. F. Runnicles; and co-workers. Pp. xi+222. (London: H.M. Stationery Office.) 10s. 6d. net. [48

Mycological Papers No. 6: Colour Terminology in Biology. By H. A. Dade. Pp. 25. (Kew: Imperial Mycological Institute.) 3s. 6d. net. [58

Department of Scientific and Industrial Research. Index to the Literature of Food Investigation. Vol. 14, No. 2, September 1942. Compiled by Agnes Elisabeth Glennie, assisted by Catherine Alexander. Pp. iv+73–150. (London: H.M. Stationery Office.) 4s. 6d. net. [128

Other Countries

Report and Accounts of the National Botanic Gardens of South Africa, Kirstenbosch, Newlands, Cape (and the Karoo Garden, Whitehill, near Matjesfontein) for the Year ending 31st December 1942. Pp. 12. (Kirstenbosch: National Botanic Gardens.) [58

Uganda Protectorate. Annual Report of the Forest Department for the Year ended 31st December 1942. Pp. 8. (Entebbe: Government Printer.) 1s. [98

U.S. Department of Agriculture. Technical Bulletin No. 848: The Ecology of the Principal Summer Weed Hosts of the Beet Leafhopper in the San Joaquin Valley, California. By F. R. Lawson and R. L. Piemeisel. Pp. 38. (Washington, D.C.: Government Printing Office.) 10 cents. [98

Catalogues

A Catalogue of Miscellaneous Books in English History and Literature printed after 1700, followed by a selection of Recent Purchases including the series of Chronicles and Memorials published under the direction of the Master of the Rolls, Fine Arts, Natural History, Works by William Prynne, Sports. (No. 612.) Pp. 40. (London: Bernard Quaritch, Ltd.) 2d.

An Improved Design of Portable Three-Cup Anemometer. (Leaflet 661.) Pp. 4. (London: C. F. Casella and Co. Ltd.)